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VOLUME 111, NUMBER 2879

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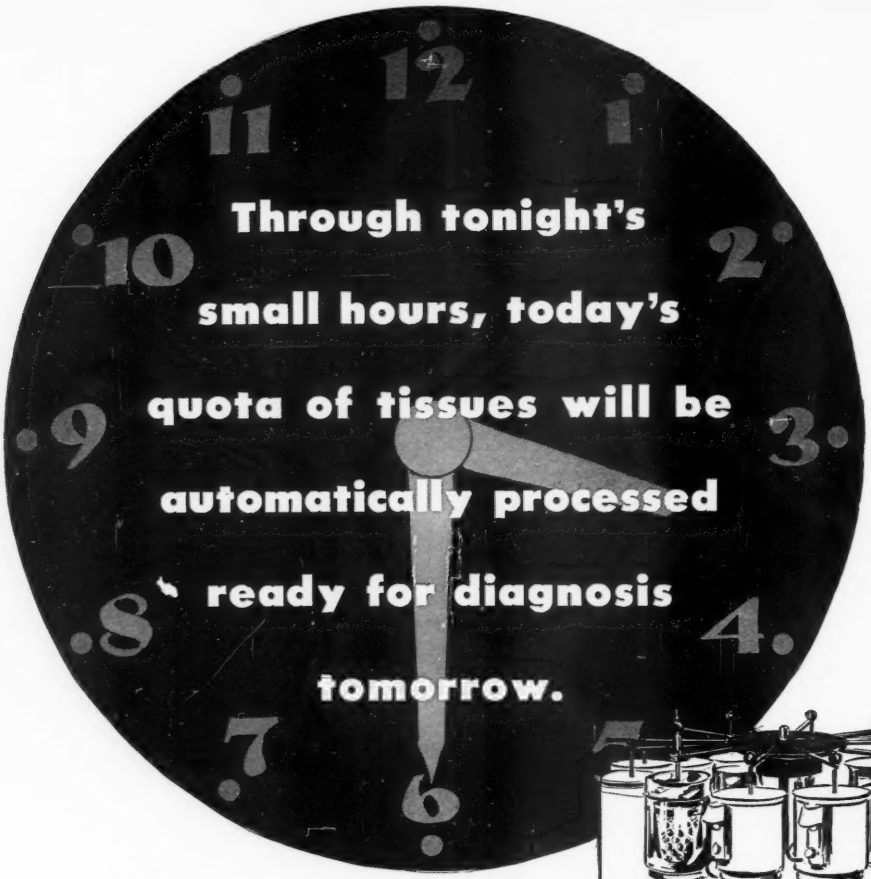

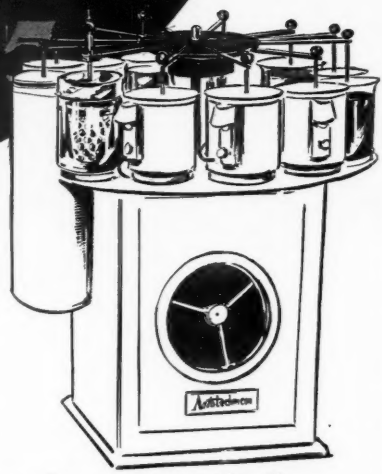

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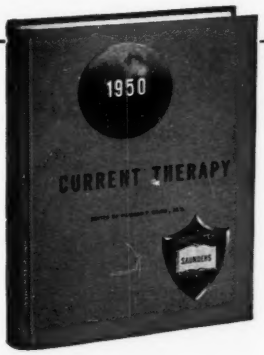

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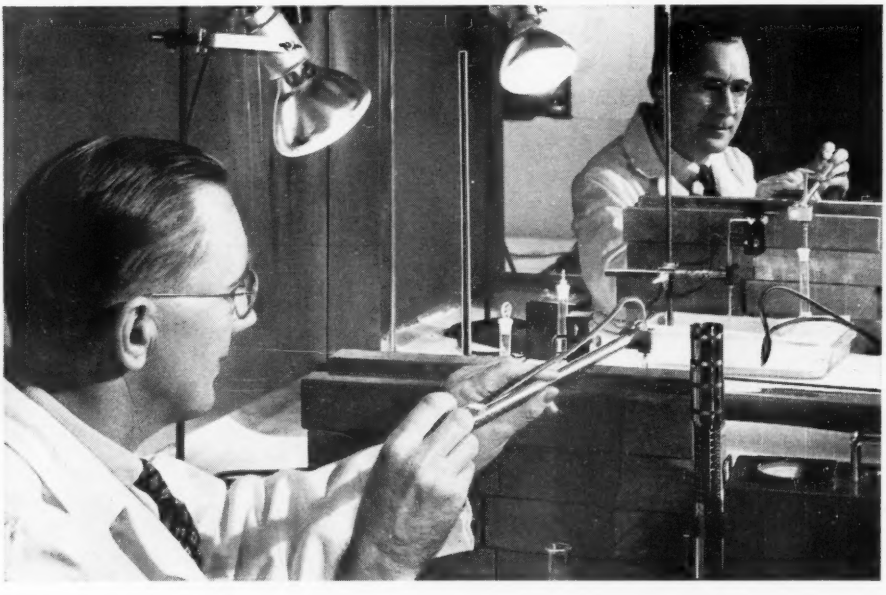

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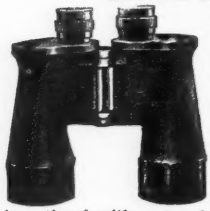

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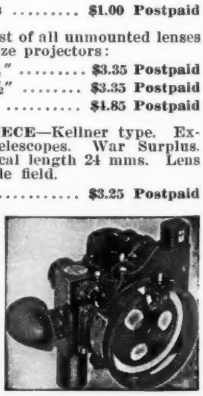

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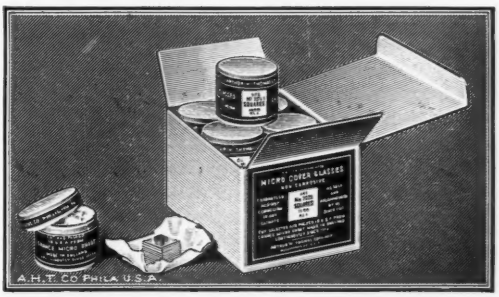

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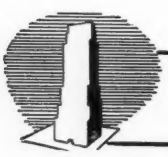

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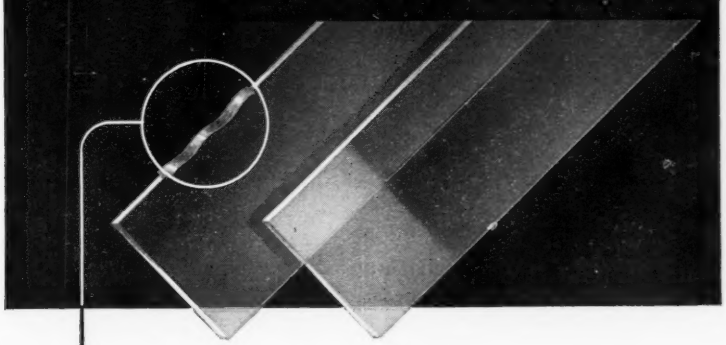

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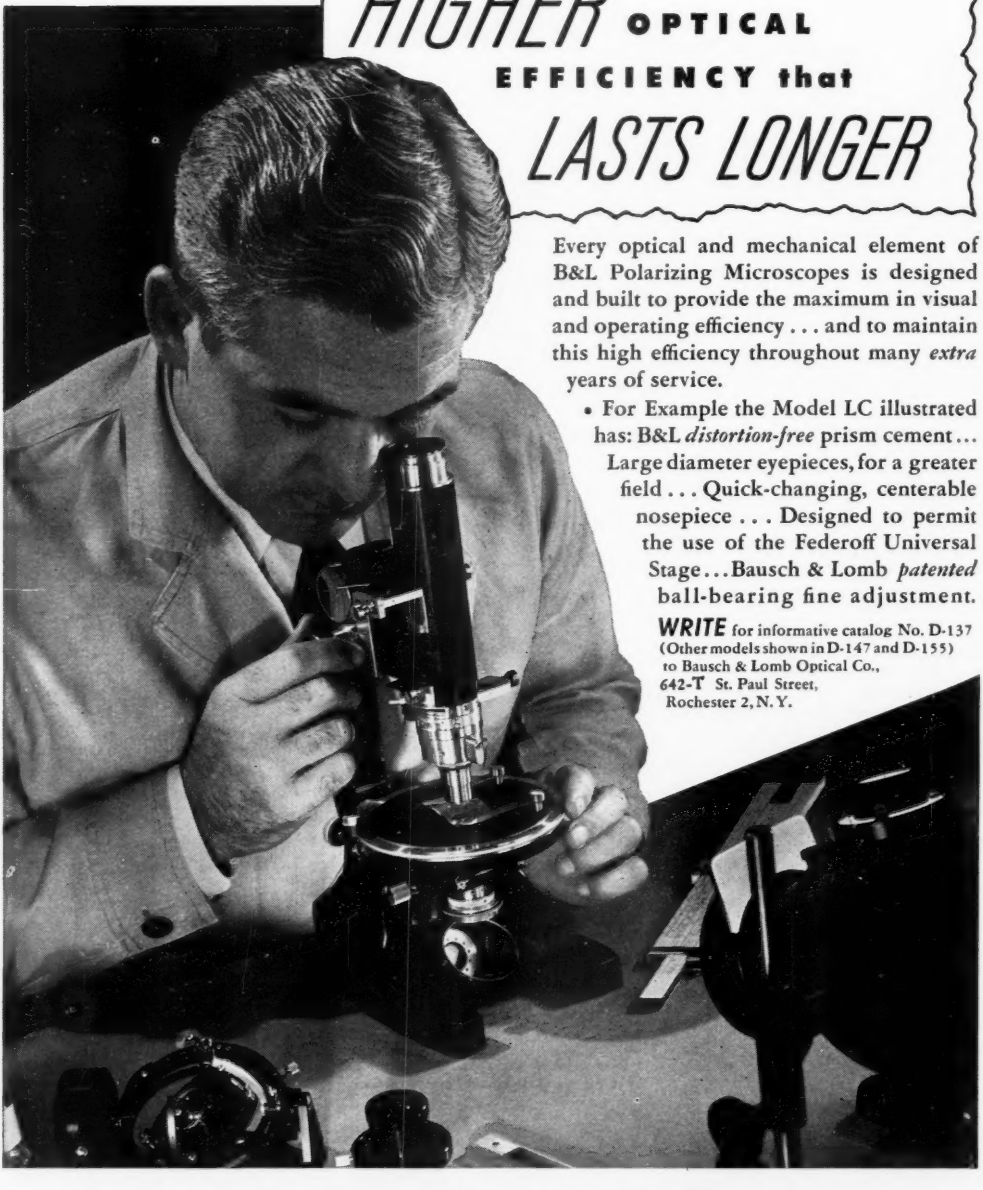

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Phosphorus in Soils and Fertilizers

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THE PROBLEMS associated with soil phosphorus, the use of phosphatic fertilizers, and the phosphorus nutrition of plants are very complex. First of all, the chemistry of orthophosphoric acid and its compounds is highly involved, and problems multiply when phosphates are introduced into a wide range of soils varying in content of clay, organic matter, lime, and particularly compounds of iron and aluminum. Additional problems arise when plants are introduced into the system, for plants vary widely in their phosphorus requirement, and in their ability to grow at different phosphorus levels. It is not surprising, therefore, that despite extensive research in this field there are many unsolved problems. These problems interest the whole American fertilizer industry, which was founded on phosphates, beginning with the manufacture of superphosphates in Baltimore in 1850. Today the industry produces approximately 10 million tons of 20 percent superphosphate or its equivalent.

An adequate supply of radioactive phosphorus is making possible extensive use of the tracer technique in soil, fertilizer, and plant nutrition investigations. This season will conclude three years of rather extensive field experiments which have been supplemented with laboratory and greenhouse studies.

Before considering results of our radioactive phosphorus investigations it may be of interest to review some of the broader aspects of soil and fertilizer phosphorus problems in the United States.

Phosphorus content of soils. The total phosphorus content of surface soils in the United States in their virgin condition is indicated by Fig. 1. The most deficient area is the coastal plain of the South Atlantic and Gulf Coasts. From this low level, the phosphorus content increases, generally speaking, to the north and west, and is particularly high in the soils of the Blue Grass Region of Kentucky and the Central Basin of Tennessee.

Total phosphorus of itself is a poor index of the phosphorus fertility status of soils, but it indicates the reserve supply, which is important. Phosphorus exists in the soil in three major forms: primary minerals, adsorbed phosphorus on the surface of clay minerals, and organic phosphorus, a constituent of the soil organic matter. The last two forms are the more important from the standpoint of immediate plant nutrition. The relative proportions of the three forms vary widely in different soils.

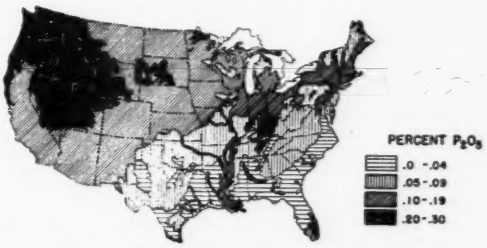

FIG. 1. Phosphoric acid in surface foot of soil.

Phosphorus is being removed from the soil by crops and is being returned in the form of crop residues, farm manures, and commercial fertilizers. The relationships between the quantity removed by harvested crops and the quantity applied as farm manures and fertilizer enables us to visualize trends in soil fertility and improvement. Fig. 2 shows this relationship by states for 1947. Note that five southeastern states and four northeastern states applied more than four times as much phosphorus as was removed by all harvested crops. Virginia and twelve other states applied between two and four times as much phosphorus as was removed by cropping. On the other hand, the Great Plains states returned less than 25 percent as much as was being removed.

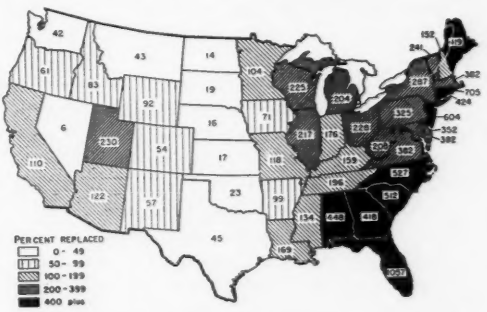

FIG. 2. Relation between phosphorus removed from the soil by harvested crops and that applied in fertilizers and manures.

This figure must be interpreted with caution. First, let's note that the heavy applications are being made in the general region where phosphorus deficiency was initially great. Conversely, the lowest return is in a region of moderate to high total phosphorus content of the soil. Second, the use of phosphorus should be related to economic return from its use rather than to the amount removed by crops. In the Great Plains, water is so often the factor limiting growth that the use of any fertilizer has been generally uneconomic. On the other hand, field experiments in the humid region have conclusively shown that it is generally profitable to apply several times as much phosphorus to tobacco, cotton, potatoes, and alfalfa as is removed by the crops. Finally, we should recognize that the degree of phosphorus replacement varies widely within states and even on different fields of a given farm.

TABLE 1

REMOVAL OF PHOSPHORIC ACID, P_2O_5, BY HARVESTED CROPS AND ADDITIONS TO SOILS AS FERTILIZERS AND FARM MANURES IN THE U. S., 1910–1947

Year	Removal by harvested crops	Applied to crops in—		
		Fertilizers	Manures	Total
	Thousands of tons			
1910	1,103	490	631	1,121
1920	1,372	640	727	1,367
1930	1,291	771	655	1,426
1940	1,439	894	615	1,509
1947	1,815	1,756	796	2,552

The relative importance of farm manure and fertilizer as phosphate materials for several years is indicated in Table 1. Manure is of minor importance in the south Atlantic and east south central regions, but is an important source in most other regions. For the country as a whole, it furnishes about one-third of the phosphorus applied to the soils. The relative efficiency of phosphorus in farm manures and in superphosphate is one of the problems we can now study critically for the first time with radiophosphorus.

Note from the table that in 1947 we returned about 50 percent more phosphorus to the soil than was removed by crops. As was indicated by Fig. 2, the return in Virginia was approximately fourfold and in several states it was even greater. Since phosphorus is not lost by leaching, these data indicate that the phosphorus content of soils in some states may be increasing. In some areas losses of added phosphorus by erosion may be serious.

A few years ago a study was made of the available phosphorus in soils used for the production of potatoes in Northampton County, Virginia. Soils from 56 farms contained an average of 662 pounds of available phosphoric acid per acre, whereas adjacent virgin soils contained only 25 to 70 pounds of available phosphoric acid. This improved phosphorus fertility status resulting from heavy fertilization raises certain questions. For example, to what extent can a farmer reduce his fertilizer bill and cash in on the residual phosphorus? Do crops differ in their ability to utilize these reserves?

By way of sharp contrast we might compare the phosphorus situation in Virginia and North Carolina with that in the Great Plains. Our research program indicates that the time is approaching when it will be economical to increase phosphorus fertilization, to offset, partially at least, the continuous removal by crops during the last 50 to 100 years.

So much for the broader aspects of the phosphorus fertility problem in this country. We will now direct our attention to the recent work with radioactive phosphorus.

Underlying principles of tracer studies. In the experiments we are to consider,[1] radioactive phosphorus, P^{32}, is used simply as a tracer. Only an infinitesimal proportion of the phosphorus in fertilizer is radioactive, but appropriate calculations enable us to determine how much of the phosphorus in a fertilized crop was derived from the fertilizer and how much from the soil. Experimental quantities of nine different radioactive fertilizers have been made at Beltsville for cooperative field experiments with state agricultural experiment stations in all sections of the country. Our experiments indicate that radioactivity per se does not influence plant growth at the levels of P^{32} used in these studies.

The experiments have included studies on (a) the comparative utilization of different phosphorus materials, (b) the utilization of soil and fertilizer phosphorus by different crops, and (c) the influence of phosphorus fertility of soils on the utilization of applied phosphorus. We shall consider some of the data from these investigations.

Comparative use of different phosphate materials. Phosphorus may be applied to the soil in any one of a wide variety of materials. All fertilizer phosphates are made from rock phosphate, which is also used for soil applications to the extent of nearly a million tons annually in recent years. Phosphorus in rock phosphate is in the fluorapatite form, highly insoluble. Superphosphate, the most commonly used phosphate fertilizer, is produced by treating rock phosphate with sulfuric acid. Its main constituents are monocalcium phosphate and calcium sulfate. The phosphorus is readily soluble in water.

[1] The data discussed were, with one exception noted, selected from twelve articles on radioactive phosphorus investigations that were published in *Soil Science*, Vol. 68, pages 113–202, August 1949.

TABLE 2

THE EFFECT OF SOURCE OF PHOSPHORUS, RATE OF APPLICATION, AND SOIL HISTORY ON THE YIELD AND PHOSPHORUS UPTAKE OF VETCH

Rate of application	Soil history		Total yield† of four cuttings	Phosphorus‡ in the plant	Phosphorus‡ derived from the fertilizer
	Treatment*	pH			
lb/acre P_2O_5			g/culture	%	%
None					
	R	4.9	8.5	0.27	...
	R + L	5.5	7.0	0.29	...
	M + L	5.8	10.1	0.28	...
	Soils treated with rock phosphate				
350	R	4.9	17.7	0.36	66.4
	R + L	5.5	15.4	0.37	46.4
	M + L	5.8	14.1	0.34	30.6
700	R	4.9	18.9	0.39	81.6
	R + L	5.5	16.4	0.38	64.6
	M + L	5.8	15.8	0.38	49.5
1400	R	4.9	19.2	0.42	94.6
	R + L	5.5	16.6	0.42	87.2
	M + L	5.8	16.6	0.40	72.9
	Soils treated with superphosphate				
87.5	R	4.9	18.3	0.33	58.0
	R + L	5.5	16.8	0.35	51.8
	M + L	5.8	17.9	0.35	50.8
175	R	4.9	18.6	0.38	79.4
	R + L	5.5	19.4	0.40	75.6
	M + L	5.8	18.2	0.39	68.3
350	R	4.9	21.3	0.46	92.3
	R + L	5.5	20.5	0.50	90.3
	M + L	5.8	21.2	0.51	86.3

* R = Residues; R + L = residues + lime; M + L = manure + lime.

† L.S.D. = 1.5.

‡ Mean of triplicates of three cuttings.

In a recently completed study, neutron pile-irradiated rock phosphate was used in comparison with superphosphate (*1*). The soils were from three plots of the Joliet, Illinois, experiment field. Their reaction varied from pH 4.9 to 5.8. Alfalfa, vetch, and rye grass were used as crops in this greenhouse experiment. The results obtained with vetch on the soil of pH 5.8 are shown in Table 2.

The first column indicates the rates of fertilization. Note the lowest rate for rock phosphate is the same as the highest rate of superphosphate. There was a good growth response to both fertilizers, but superphosphate was distinctly superior to rock phosphate. Fertilization increased the percentage of phosphorus in the plant. If this is used as an index of efficiency, superphosphate is again the superior source.

The last column of the table supplies data obtained by the use of P^{32}. It shows that the vetch grown with increasing rates of rock phosphate derived from 30 percent to 73 percent of the phosphorus from the fertilizer. The corresponding figures for superphosphate were 50 percent to 86 percent.

The results obtained with four successive cuttings of alfalfa are shown graphically in Fig. 3. It illustrates several points. First, we should note that in this figure the comparison is between 700 pounds of phosphoric acid in rock phosphate and 150 pounds of phosphoric acid in superphosphate. The figure shows that the utilization of rock phosphate declined with increasing pH, but the utilization of superphosphate was not materially affected by the soil reaction. At the rates compared, rock phosphate was superior to superphosphate on the most acid soil at all harvests. Superphosphate, however, was superior to rock phosphate on the least acid soil. On the soil

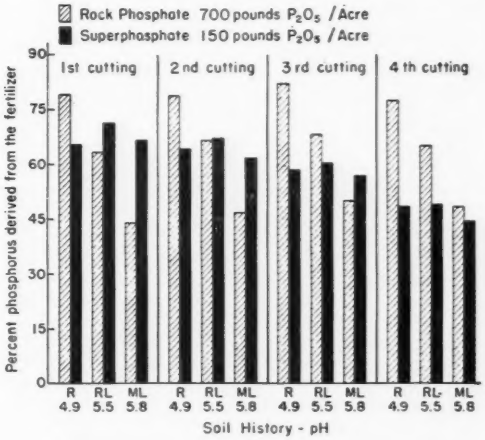

FIG. 3.

of intermediate acidity, superphosphate was superior on the first two cuttings and rock phosphate on the last two. The experiments noted here show influences of the different phosphorus carriers for a single season only. Residual effects as shown in crop yields over a period of years must also be considered. Irradiated materials are not suitable for such studies.

The field experiments have included the common phosphate materials produced by the fertilizer industry—superphosphate, double superphosphate, ammonium phosphate, and liquid phosphoric acid. They also included dicalcium phosphate, calcium metaphosphate—a TVA product—and alpha tricalcium phosphate. The last named is the phosphate compound in the furnace products produced by Coronet Phosphate Company and TVA.

The results of an experiment with three materials for corn grown on an acid soil in North Carolina are given in Fig. 4. This shows that the crop obtained twice as much phosphorus from superphosphate and

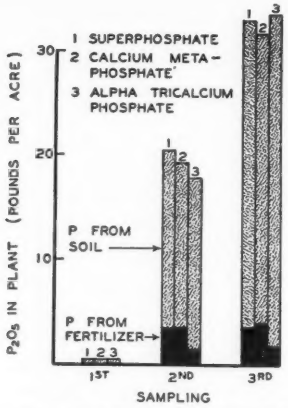

FIG. 4. Corn—Norfolk series (412 lb P_2O_5/acre).

calcium metaphosphate as from alpha tricalcium phosphate. It may be noted that most of the phosphorus in the crop came from the soil on all treatments. The supply of native soil phosphorus in much of this area is low. It may be assumed, therefore, that a large part of the so-called soil phosphorus taken up is in reality from fertilizer residues of former years.

An experiment in Iowa with oats as a test crop indicated a slight superiority of superphosphate. There were no significant differences between the other three materials. In this experiment, fertilization

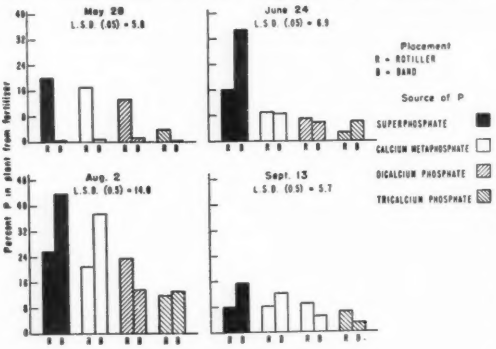

FIG. 5.

materially increased the phosphorus content of the crop. A moderately large percentage of the phosphorus was derived from the fertilizer.

The last experiment with different materials was with sugar beets in Colorado. It involved two methods of fertilizer placement. In the first, the fertilizer was mixed in the seedbed with a rototiller and the seed planted in the fertilized soil. In the second, the fertilizer was placed in a band six inches from the seed. The results are shown in Fig. 5.

The first bar of each pair represents the percentage of phosphorus in the plant derived from the fertilizer when the rototiller placement was used. At all sampling dates superphosphate was superior, but calcium metaphosphate was a close second.

The effect of placement is of interest. At the first sampling the beet roots apparently had not reached the band of fertilizer. At the second and subsequent samplings the band placement resulted in the better utilization of superphosphate and calcium metaphosphate.

Our studies with materials indicate the general superiority of superphosphate under most conditions. Canadian workers, however, have some evidence that ammonium phosphate is superior to superphosphate for wheat on calcareous soils.

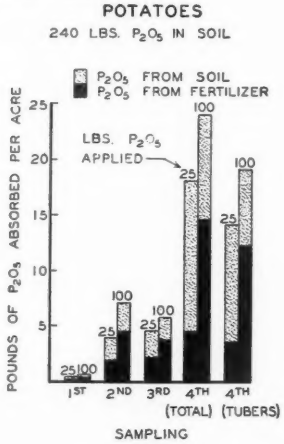

FIG. 6.

Earlier mention was made of the importance of farm manures as a source of phosphorus. Cornell investigators have compared farm manure and superphosphate, both containing P^{32}, in greenhouse experiments. Their data indicate that the phosphorus in the two materials is utilized to approximately the same degree by rye grass. Somewhat similar results were obtained at Beltsville in greenhouse experiments comparing green manures and superphosphate as sources of phosphorus for rye grass. In this instance, green manures supplied about 70 percent as much phosphorus to the rye grass as did superphosphate. The preliminary work, therefore, indicates that the phosphorus in green manures and farm manure is approximately as efficient as phosphorus applied in superphosphate.

Utilization of applied phosphorus by different crops. The experiments indicate that crops differ widely in their capacity to utilize soil and fertilizer phosphorus. The results of an experiment with potatoes are given in Fig. 6, in which the amount of phosphorus absorbed

per acre is plotted against four sampling dates. The experiment was on a soil containing 240 pounds of available phosphorus per acre, which is a moderate quantity. In the experiment potatoes were fertilized with 25 and 100 pounds of phosphoric acid per acre. The bars show the amount of phosphoric acid in the crop at each sampling date. The solid black portion of each column indicates phosphorus derived from the fertilizer, whereas the shaded portion represents phosphorus from the soil. Note first that at all sampling dates and at both rates of fertilization almost one-half of the phosphorus was from the fertilizer, second that increasing the rate of fertilization increased both the total and fertilizer phosphorus on sampling dates except the first, and third that at final harvest only 15

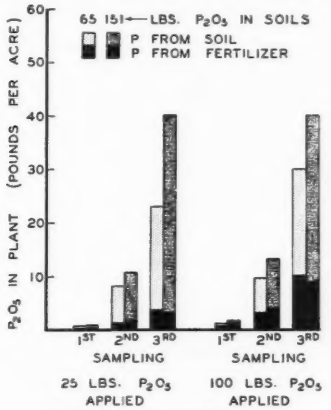

FIG. 7. Corn—Bladen series.

percent to 25 percent of the applied phosphorus was found in the crop. A similar experiment with corn affords an interesting contrast (Fig. 7). Here we have two rates of fertilization—25 and 100 pounds—just as in the potato experiment. We also have two soils—one with 65 and the other with 150 pounds per acre of available phosphoric acid. Neither soil is as fertile as that in the potato experiment, which had 240 pounds per acre of available phosphoric acid. It is immediately evident that, after the first sampling date, corn obtains most of the phosphorus from the soil. The total phosphorus absorbed is influenced more by the initial level of soil phosphorus than by the rate of fertilization. Finally, recovery of fertilizer phosphorus is lower than in the case of potatoes.

A comparison of the utilization of fertilizer phosphorus by corn, soybeans, and potatoes is given in Fig. 8. With corn the percentage of phosphorus derived from the fertilizer dropped from 80 percent to 25 percent from the first to the last sampling. It is interesting to note that potatoes at all sampling dates derived 50 percent to 60 percent of the phos-

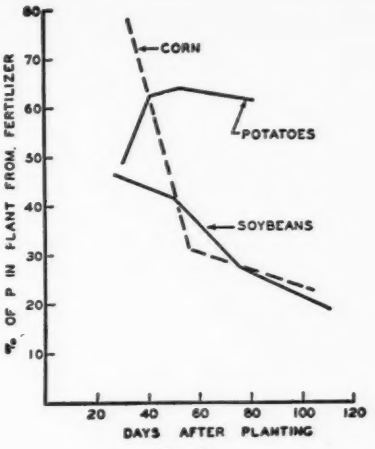

FIG. 8.

phorus from the fertilizer. Soybeans were similar to corn except that the initial utilization of fertilizer phosphorus was not nearly as great. In this connection it may be noted that the initial response of corn to phosphorus fertilization was much greater than was the case with soybeans. By the end of the growing

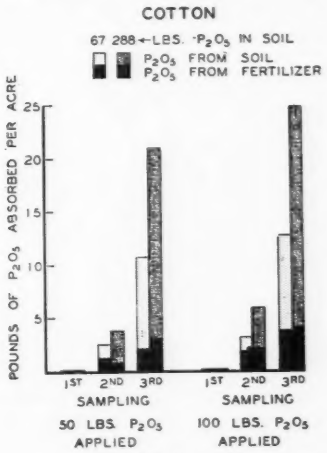

FIG. 9.

season, however, neither crop showed a significant yield response from the phosphate fertilization.

These and other experiments clearly indicate that potatoes absorb most of their phosphorus from a limited volume of soil and that they utilize applied phosphorus more fully than most crops. Corn initially draws on the band of fertilizer but very rapidly extends its root system and obtains most of its phosphorus from the soil. From this we may assume that corn would utilize the accumulated phosphorus in well-

fertilized soils more effectively than would potatoes, cotton, or tobacco. Tobacco uses fertilizer phosphorus in the seedling stage more than cotton. Both appear less efficient than corn in using soil phosphorus.

Influence of phosphorus fertility of soils on the utilization of fertilizer phosphorus. In an experiment with cotton, 50 and 100 pounds of phosphoric acid were applied as superphosphate on two soils. One soil contained 67, the other 288 pounds of available phosphoric acid per acre. The data are given in Fig. 9. The striking thing about the figure is that the really big difference in the phosphorus content of the crop is due to the difference in the two soils. The phosphorus fertility level of the soil did not materially influence the utilization of fertilizer phosphorus. Similar results have been obtained with both corn and potatoes. The potato experiments were on Long Island and in North Carolina. At both locations three rates of phosphorus fertilization were compared on two soils of widely different phosphorus fertility. Here again, as with cotton and corn, the phosphorus content of the soil did not materially influence the utilization of fertilizer phosphorus when applied in bands. The soil fertility level, however, did greatly influence the amount of soil phosphorus used, and the total phosphorus absorbed by the crop.

Such data give convincing evidence of the importance of increasing the phosphorus fertility status of soils to a reasonably good level. They also indicate that the various crops studied used to good advantage residual phosphorus from previous fertilization.

In conclusion, we feel that radioactive phosphorus is a very useful tool for certain studies. It has its limitations, however; one is that the rate of decay is such that it can be followed only one season. The residual effects of phosphorus fertilizers, therefore, have to be determined by less direct methods. We are also conducting, as supplementary to this field approach and closely integrated with it, fundamental investigations on plant nutrition, soil chemistry, and soil-fertilizer reactions, both rapid and long term. Our knowledge of soil and fertilizer phosphorus is steadily increasing. It appears that, for the most part, we are gradually increasing the phosphorus fertility status of our soils. If the present high level of fertilizer phosphate use is maintained, and somewhat better distribution is achieved to certain sections of the country, phosphorus will gradually become less of a limiting factor in crop production.

Reference

1. Fried, Maurice and MacKenzie, Arnold J. *Proc. soil sci. Soc. Amer.*, to be published.

Based on a paper presented at a meeting of the Virginia Section of the American Chemical Society, at Petersburg, Virginia, October 20, 1949.

The First Year of the SCLP

The Scientists' Committee on Loyalty Problems,[1]

Princeton, New Jersey

IN THE FALL OF 1948 a committee of the Federation of American Scientists was formed to deal with the security and loyalty problems confronting scientists. This report summarizes the activities of that committee, the SCLP, in its first year.

In order to make the committee's operation more effective, members have been chosen from a fairly small geographical area. The committee has a panel of 80 sponsors and consultants, however, who are broadly representative, geographically and professionally.

Because other groups were studying the long range legal (*1*) and sociological (*3*) implications of the security program, the SCLP has concentrated its attention on the immediate and practical problems facing scientists. The first action of the committee was to collect and study the available information on clearance procedures (*13*). It then undertook to obtain for individuals, without judging the merits of any case, the full protection of existing regulations. And on the basis of this knowledge and experience, it has urged upon government agencies the adoption of specific reforms and procedures.

One point deserves particular emphasis at the outset. Although the SCLP has worked consistently for more equitable and judicial clearance procedures, it does not believe that a poor security risk should be given clearance. The steps it has taken and the measures it has recommended are all designed to ensure

[1] Present members of the committee are: Lyman Spitzer, Jr., chairman; William A. Higinbotham, associate chairman; Arthur S. Wightman, secretary; Donald R. Hamilton, treasurer; Kenneth W. Ford, Samuel A. Goudsmit, Herbert R. Muether, T. Alexander Pond, and Irving Wolff.

a fair hearing of all the available evidence; they are *not* designed to increase the number of clearances given in cases that are actually doubtful.

Aid to Individuals

Because it never occurs to them to doubt their own acceptability, most scientists are poorly informed on clearance procedures. The local clearance board may do little to relieve the resulting confusion, since the security procedures within an agency are likely to be variable, and subject to different interpretations because of the lack of precedents. Hence, the first need of a scientist with clearance difficulties is often for procedural information. To serve this need, the SCLP keeps an up-to-date summary of loyalty and security clearance procedures of the various government agencies (*13*) available on request. These procedures vary widely from agency to agency.

If clearance has been initially refused a scientist, and if he is allowed an appeal, the SCLP has in some cases been able to secure legal advice for him. Finally, in cases where a decision has been left pending overly long, the committee may attempt informally to speed the decision.

Current appeal procedures are far from perfect, as will be pointed out later in some detail. Even so, an appeal of any sort offers at least some opportunity to prevent the perpetuation, in an uncontroverted form, of the flimsy and hearsay evidence which secret dossiers sometimes contain. There is a large group of individuals for whom no appeal is possible. This group is composed of prospective employees, i.e., scientists to whom a job in a classified project has been offered pending clearance. It is presently possible for such a man to be barred indefinitely from many types of government work on charges of which he is ignorant, and on evidence which he is not allowed to dispute.

When an appeal is possible, the scientist denied clearance may actively seek a reversal of the decision in order to keep his job or to clear his name for future jobs. Or he may feel that the expense and trouble of appeal are not worth the effort, and seek employment where clearance is not required. This point of view could represent a threat to the health of government-sponsored research. Our case histories indicate that a number of scientists in fact feel this way.

The committee has considered 62 individual cases in its first year. About one third of these were prospective employees, mostly with the Atomic Energy Commission. The rest were employees divided among the various military departments and their contractors, and the AEC and its contractors, with a few in other departments, such as the Department of Commerce and the Veterans' Administration. Twenty-seven of these cases are now closed, either by a final decision of a review board, or because no appeal was possible, or because the scientists concerned preferred to drop the whole matter and seek jobs elsewhere. (Not all of the 20 prospective employees fall in this group. Some still seek action, despite the present official hopelessness of their situation.) The other half of the cases studied are still open, and some have been pending for over a year.

The committee has gained information from these cases which has been very helpful in formulating advice for others in clearance difficulties. Two general rules have emerged for the scientist facing a clearance hearing or appeal: (1) get a lawyer, and (2) attend the hearing personally. Unfortunately, these rules are not always financially practical.

A specific case may best serve to illustrate the problems met by scientists and the way in which the SCLP has endeavored to help:

The case of Mr. K. Mr. K. worked in Washington as a scientific aide for the Navy during two war years. After two subsequent years in the Navy as a radar operator, he returned to a Navy job in Washington, then took a leave of absence to finish his undergraduate college training in science. A few weeks before graduation in January 1948, he was accepted for technical employment on an AEC contract at L. University. This acceptance was contingent upon clearance by the AEC, which was expected to take "at least a month." The employment was to begin as soon as clearance came through.

In February 1948, Mr. K. was informed that his fingerprints had been lost and that a new set was needed. He had a set made and forwarded them to L. University. In March he wrote to find out if any action had been taken. Mr. M. at L. University replied that word was expected "momentarily." In June Mr. K. wrote again, and was told by Mr. M. that word was expected "in a short time."

On July 1, Mr. M. wrote:

> The Federal Bureau of Investigation has made a thorough study of your case and the file has finally reached the security officer of the Atomic Energy Commission in the New York Office. Because of some information presented in the FBI report, the New York Office is unable to grant clearance. However, they do not want to take the responsibility of refusing clearance, and at our request they will send your papers to a Board of Review in Washington for final decision. At best it will be another three months before the Board reaches your case and comes to a final decision.
>
> I offered you a position on our project with the provision that you first receive clearance from the AEC. Until clearance is actually refused, I feel obliged to hold a position open for you. However, the security officer in New York has informed me that the chances of a favorable decision from the Board of Review are slim.

You have waited five months now for clearance and will have to wait about three months more with no assurance that you will have a job even then. If you wish, I will request New York to send your papers on to Washington. However, you may prefer to drop the matter at this stage and accept a position elsewhere. If, in spite of the slim possibility of a favorable outcome, you wish to press for a final decision, please let me know and I will see that the necessary steps are taken.

Mr. K. answered promptly:

My conscience is clear. I most certainly do wish to press for a final decision. Please request New York to send my papers to the Board of Review.

On November 9, nearly 10 months after the first request for clearance, Mr. M. wrote:

I regret to inform you that the Atomic Energy Commission has refused to grant clearance in your case, and the offer that I made to you some time ago must be withdrawn. No reasons were given for the refusal of clearance, and since the opinion was handed down by the review board in Washington, I am afraid there is nothing further we can do.

Mr. K. in the meantime had accepted a fellowship at N. University and was working on his M.A. He was anxious, however, to press for clearance, since he might someday wish to work on an AEC contract. Acting on SCLP advice, Mr. K. wrote to the New York office of the AEC to request a statement of charges and an opportunity to defend himself. Mr. O. of the AEC replied on August 26, 1949:

In reply to your letter of August 11, 1949, . . . I should like to say at the outset that the AEC did not refuse to grant you clearance for that position. Your case was never pursued to the point where a final decision was reached by the AEC one way or the other concerning your eligibility for security clearance. . . .

The letters of Mr. M. and Mr. O. were thus directly contradictory. After two years of waiting, Mr. K. still hopes to be cleared for work on AEC contracts.

The case of Mr. K. is not typical. No two cases are alike. But common to most cases that have come to the committee's attention are red tape, confusion, and delay. Personnel offices have been known to tell applicants that clearance was refused when in fact no clearance was involved, whereas other applicants have been turned down for clearance reasons without being told that their clearance had given trouble.

The Issues

The security problem is the direct result of the continuing international tension, and the national security must, of course, be the overriding consideration in any discussion of this or related problems. Given this premise, there are still two difficult questions to consider:

(1) To what extent must secrecy be carried in scientific matters best to protect the national security?

(2) How shall decisions on the potential disloyalty and reliability of a particular scientist be made?

At the beginning of its work, different members of the committee had widely different answers to these two questions. The experience gathered during the year has produced fairly uniform agreement on certain general answers.

Boundaries of secrecy. Selection of those items (facts, equipment, research projects) which must be kept secret to ensure the national security is the touchstone in the formulation of a security program. Once the boundaries of necessary secrecy are defined and removed from the discussion, the issues become clear. The remaining problem is one of personnel only. The SCLP believes that the boundary should be located by balancing the requirements of security by secrecy against those of security by achievement, that is, by evaluating the point where restriction of information so hampers research that national security is more harmed than helped. For example, vital secrets in atomic energy can be adequately protected for the time being by classifying the technology and status of bomb construction. However, extension of secrecy into the field of nuclear physics would be unwise in the long run. Any security gained by general secrecy of our *fundamental* data would be rapidly overweighed by the diminished vigor of our own research. These remarks will be accepted as truisms by most members of the scientific community; but the point is worth making in view of the widespread tendency among the laity to regard "science" and "secrets" as synonymous. The committee expressed its opposition to the unwarranted spread of secrecy in a letter to Senator Brien McMahon, chairman of the Joint Committee on Atomic Energy, on February 28, 1949.

Clearly, no committee composed only of scientists (or, even more clearly, only of military men, or only of government executives) is qualified to determine accurately which phases of research are vital to defense. For this reason, the SCLP has accepted the boundary as it found it, and has confined itself to the remaining problems: first, the scope, and second, the mechanics of the personnel security programs.

Scope of the Personnel Security Program. Given a boundary between secret and nonsecret research, the question arises, how shall the investigation of personnel be related to this boundary? It is clear that such investigations must include, as a minimum, all scientists at work in the secret fields. However, the committee feels that any extension of personnel se-

curity measures beyond the minimum and into the fields of general science threatens the national security rather than strengthens it. Three principal reasons are:

1. Investigation of personnel on attitudes and associations represents a break with the traditions of democracy, and should be tolerated only when clearly essential to the national security.

2. The clearance requirements, especially as they have been applied in the past, deter some capable and much-needed scientists from going into important scientific work.

3. There is some danger that personnel investigations in nonsecret fields may be followed by extension of secrecy into those fields.

The AEC fellowship controversy in the spring and summer of 1949 provided an arena for the airing of views on the scope of the security program, and an ideal one in the sense that no question of national security was directly involved. The controversy was set off by the discovery of an AEC fellow who was a professed communist. It culminated in the passage of the O'Mahoney amendent to the Independent Agencies Appropriation Bill (H.R. 4177) in August 1949, requiring full FBI investigation and AEC clearance of all AEC fellows. It was clearly a question of extension of investigations into nonsecret areas, since the fellowship in no way implied security clearance or any commitment to work for the AEC. In a letter to Senator McMahon dated May 23, 1949 the committee took a strong stand against this extension, using the arguments just stated, and in addition pointing out that recent history has shown clearly the damage that can be done to scientific research by the requirement of political orthodoxy. The committee further urged scientists to make known their views to the senators and congressmen.

On October 26, 1949, the Council of the National Academy of Sciences, whose National Research Council administers the AEC fellowship fund, stated its position on the program as amended:

> In our opinion the requirement of FBI investigation and Atomic Energy Commission clearance is ill-advised for those fellows who neither work on secret material, nor are directly preparing for work on Atomic Energy Commission projects. We are convinced that by this restriction the value of the broad program has been greatly reduced; we have grave doubts whether the continuance of the Atomic Energy Commission Fellowship Program thus restricted is in the national interests.

Although it was clearly the intent of Congress to extend investigations to cover all prospective AEC fellows, regardless of their fields of research, the previous security boundaries were restored by the announcement of the AEC, dated December 16, 1949 (*9*), of the withdrawal of its fellowship program, except for postdoctoral grants for secret work. This step was taken after consultation with the National Academy of Sciences.

Since this article was prepared, arrangements have been made by the AEC to resume its predoctoral fellowship program, but with the restriction that "the subjects of research must be sufficiently closely related to atomic energy to justify a presumption that the candidate, upon completion of his studies, will be especially suited for employment by the Atomic Energy Commission or one of its contractors." The program will be administered not by the National Research Council but by four different agencies: Associated Universities, Inc. (which operates the Brookhaven Laboratory) for the northeastern region; Oak Ridge Institute for Nuclear Studies for the southeastern region; and by fellowship boards under contract with universities for the midwestern and western regions.

In a letter to the SCLP, Senator McMahon noted that a National Science Foundation might be established, and that this organization might administer fellowships for nonsecret research. If clearance requirements could be kept out of all nonsecret work sponsored by the NSF, this organization could be the answer to the fellowship controversy. In addition, the support of basic research by an agency outside the National Defense Establishment might help prevent further inroads of personnel security into nonclassified research. However, if legislation setting up the NSF requires full FBI investigation and clearance for many workers in nonsecret areas, a large number of scientists will oppose the adoption of such a bill.

Mechanics of the Personnel Security Program. The second major problem of personnel security is the practical one. How shall a loyal person or a "good security risk" be determined? How can the potentially dangerous person be discovered and removed without harming the innocent? What procedures can provide a maximum of security with a minimum of injustice?

Whatever else they may be called, security hearings are trials in which probability of innocence or guilt is determined; and denial of clearance can do more lasting harm than the mere loss of a job. Yet there is an important difference between security hearings and criminal trials, a difference of emphasis. Both should attempt to convict the guilty and protect the innocent. But the underlying philosophy of the criminal court is: Better to let a guilty man go free than to convict an innocent man. The philosophy of the security program would appear to be rather: Better to deny clearance to a reliable man than to

clear a disloyal man. Even within this assumption, much can be done to reduce the possibility of an incorrect decision. The SCLP believes that all of its suggested reforms increase the probability of a reliable man's securing clearance without in any way decreasing the probability of clearance's being denied to a poor security risk.

Suggested Reforms

The committee's proposals for procedural reform in the security program are the following:

1. A hearing should be held by an independent civilian board before the individual is dismissed.

2. An appeal hearing should be held before a different but similarly constituted board.

3. Hearings, as such, should be unclassified.

4. A detailed statement of the charges against the individual should be made, so that he has an adequate opportunity to prepare his defense.

5. Permission should be given for confrontation and cross-examination of witnesses, except when concealment of identity of informant is necessary to the carrying out of further investigation.

6. In all cases, an unclassified transcript of the hearing should be supplied to the individual.

7. Written judgment should be presented to the individual with all possible speed.

No agency lacks all of these procedural safeguards, and no agency incorporates them completely. On November 14, 1949, the committee recommended to the heads of the military agencies the reforms that were respectively applicable as of that date. These recommendations, with the replies received by the committee, give a picture of the current state of security procedures.

Department of the Army. The Department of the Army lacks safeguards numbers 1, 2, 5, and 6. In a letter to the SCLP dated November 21, 1949, the Office of the Secretary of the Army stated that no immediate changes in security regulations were contemplated.

Department of the Navy. In a letter dated January 6, 1950, the Secretary of the Navy advised the SCLP of the policy of his department in regard to recommendations 1, 2, 4, 5, and 6.

1 and 2) The department is not required by law to grant a hearing. However, it has provided that an employee discharged on security grounds will be granted a hearing before the Navy Department Loyalty Appeal Board in Washington, D. C.

4) After an employee has been removed, he is given, on request, a statement of charges.

5) Permission to confront and cross-examine witnesses is denied in general in order to safeguard sources of information.

6) A transcript of the hearing before the Loyalty Appeal Board is given the former employee.

Department of the Air Force. The Air Force regulations lack safeguard 5. However, in a letter to SCLP dated December 8, 1949, the Office of the Director of Civilian Personnel, USAF, stated that cross-examination of individuals who give derogatory information and who appear as government witnesses is permitted, and that it is the usual practice to call all such individuals willing to testify. The letter points out, however, that such individuals cannot be forced to appear, and that an informant's desire for anonymity is respected.

Department of Defense. The SCLP has, in addition, urged the Secretary of Defense to unify the procedures of all departments under his jurisdiction. A letter from Secretary Forrestal in February 1949, stated that the whole security program was under review at that time in the Department of Defense. In May and November, 1949, Secretary Johnson promised continued attention to the clearance procedures in his department.

The Industrial Employment Review Board (IERB). The IERB, an appeal board administered by the Munitions Board for contractors of the Army, Navy, and Air Force, lacked most of SCLP's recommended safeguards until recently. In a letter dated December 5, 1949, the chief of the Munitions Board's Office of Manpower advised the committee of a reorganization of the IERB, in which safeguards 4, 6, and 7 have been included. Policy on the remaining suggestions is as follows:

1. The boards will be mixed civilian and military personnel, with a civilian chairman.

3. The hearings cannot be unclassified or opened to the public; however, participation of the appellant's attorney, union representative, and any witnesses is allowed.

5. As with the Air Force, cross-examination of only those witnesses that the government chooses to call is allowed.

The committee does not understand, in respect to IERB policy on recommendation 3, why it is necessary, or indeed how it is possible, to introduce classified information into a hearing involving an individual who has not been cleared. If *no* classified information is introduced, then there is no reason to classify the hearing. The IERB is the only agency that finds classification necessary. However, the rights of appellants are greatly enhanced by the changes that have been made.

The AEC. The AEC lacks safeguard 5, but has on occasion allowed confrontation.

Proposal 5, recommending permission for the cross-examination of adverse witnesses, is not included in

full rigor in the procedures of any security board. Committee files indicate that weak or irrelevant evidence is used in some hearings and in some charges. The SCLP has pointed out (*10, 11, 12*) that scientists themselves may help to improve this situation by signing all evidence given to FBI investigators and offering to appear in person to testify.

Criteria for Clearance

Determination of the actual criteria to be applied in determining eligibility for security clearance is apparently extremely difficult. Very recently the AEC (*4*), the Air Force (*5*), and the IERB (*6*) have formulated such criteria. The formulation was so difficult that at one point the AEC despaired of ever spelling it out in specific terms (*8*). However the committee believes that similar codifications should be made by all agencies concerned.

The AEC and Air Force recognize two types of security risk. For example (*3*):

"Category (A) includes those classes of derogatory information which establish a presumption of security risk . . .," that is, disloyalty (in the sense of the President's loyalty clearance order (*7*), felonious conduct, insanity, violation or disregard of security regulations, etc. In these cases, refusal of clearance is mandatory (subject to appeal in Washington).

"Category (B) includes those classes of derogatory information where the extent of activities, the attitudes or convictions of the individual must be weighed . . . or [those of] his spouse. . . ." In these cases, clearance can be granted or denied, or referred to Washington. Much has been written elsewhere on these types of guilt (*1, 2, 3*). The committee, however, has confined itself to opposing any unwarranted increase in the area where these criteria must be applied.

The Problem of Prospective Employees

In urging procedural reforms, the SCLP has consistently asked that they be extended to prospective as well as actual employees. The complete lack of procedural safeguards for most prospective employees is probably the outstanding defect of the security program for scientists today. Excepting only the Civil Service Commission (loyalty program) and the Air Force, the agencies principally affecting scientists do not allow a prospective employee either a hearing or an appeal. These agencies are the Navy, the Army, the IERB, and the AEC.

This undesirable situation will become increasingly important as the proportion of prospective employees dealt with in security cases steadily grows. Enlightened national self-interest demands that withdrawal of a job offer on security grounds be handled with whatever degree of care is accorded to the process of firing on security grounds. When a prospective employee is denied clearance, this fact appears on his FBI record. As a result, it then becomes administratively difficult for anyone else to employ him on government or other classified work at any time. This FBI record may even imperil his chance of obtaining a good position of any sort, in or out of government, classified or unclassified. It is clearly unfair to risk blackening a man's record and threatening his entire subsequent career without establishing some procedural safeguards.

The SCLP has pointed out the serious inequality in the treatment of prospective and actual employees to the Army, the Navy, the Air Force, the AEC, and the Joint Committee on Atomic Energy. Only the Air Force has changed its policy to remedy this inequality. In a letter to SCLP in December 1949, the Air Force revealed that its prospective and actual employees were now granted the same treatment in security cases. Some evidence of a possible change in policy towards prospective employees has come from the AEC. The commission stated over a year ago in its fourth semiannual report (July 1948) that applicant hearings were "currently under consideration." In a subsequent interview (*8*), however, the commission was unenthusiastic about such hearings. The principal arguments against them at that time were the additional trouble and expense they would make for the AEC, which already found security investigations a considerable burden.

So long as the present situation continues, the least that can be done is suitably to warn prospective employees against overoptimism. Plain honesty demands that an applicant to whom a job offer is made be informed that hitches sometimes arise in clearance, that personal plans made on the assumption of future clearance may go awry, and that there is no way in which a decision may be appealed or a black mark erased.

Conclusions

The experience of the SCLP during its first year of operation, which has been outlined in the present report, has led to the following conclusions:

1. *Further reforms in procedure are desirable.* Although relatively few scientists encounter clearance difficulties, and clearance procedures have been substantially improved during the past year, there is further room for improvement. In particular, procedural safeguards for prospective employees are needed. Also, a more rapid processing of individual cases is desirable to eliminate long delays.

(Continued on page 238.)

Technical Papers

Stars in Photographic Emulsions Initiated by π^- Mesons

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Stars initiated by π^- mesons in photographic emulsions have been examined in order to determine the probability that such a meson will form a star of a specified number of prongs. Preliminary results have been previously published in abstract form (*1*). Recently, Perkins (*13*) has made a similar study using cosmic ray data. When account is taken of his different method of tabulating recoils, his distribution is compatible with that given in this paper.

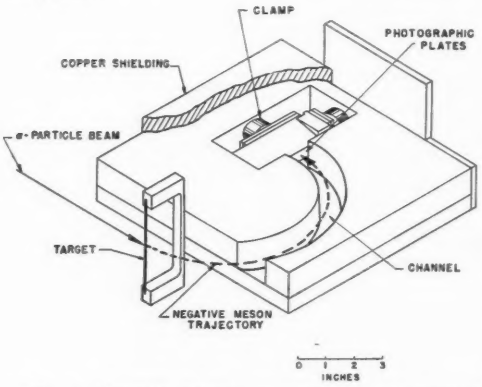


FIG. 1. Schematic diagram of apparatus for observing π^- mesons.

It may be noted that these results are potentially useful in connection with the possible existence of mesons of mass different from that of either π or μ mesons, since a prong spectrum is expected to be characteristic of each meson type.

The opportunity to study a group of π^- mesons with essentially no contamination by μ^- mesons was afforded by the determination of the mass of the π^- meson which was made in this laboratory (*2*, *7*, *10*). The apparatus, shown in Fig. 1, selects a group of mesons in a small momentum range. The channel was designed to eliminate π^- mesons of undesired momenta as well as positive mesons, and to minimize the number of μ^- mesons. For this work Ilford C.2 emulsions were used. The mesons were measured by the method of $H\rho$ and range described in earlier reports (*2*, *7*, *8*).

In order to estimate the μ^- contamination, the prong spectrum of those mesons in the central part of the distribution of meson masses was compared with the spectrum of the remainder of the group. Since the μ^- mesons would be products of the decay of π^- mesons in flight, their apparent mass values are expected to be distributed throughout the entire mass spectrum. However, it was found that these two prong spectra agreed within the probable errors. From these results and the assumption that μ^- mesons do not initiate stars (*5*, *9*), it is concluded that no more than one percent of the mesons included in the total prong spectrum are μ^- mesons.

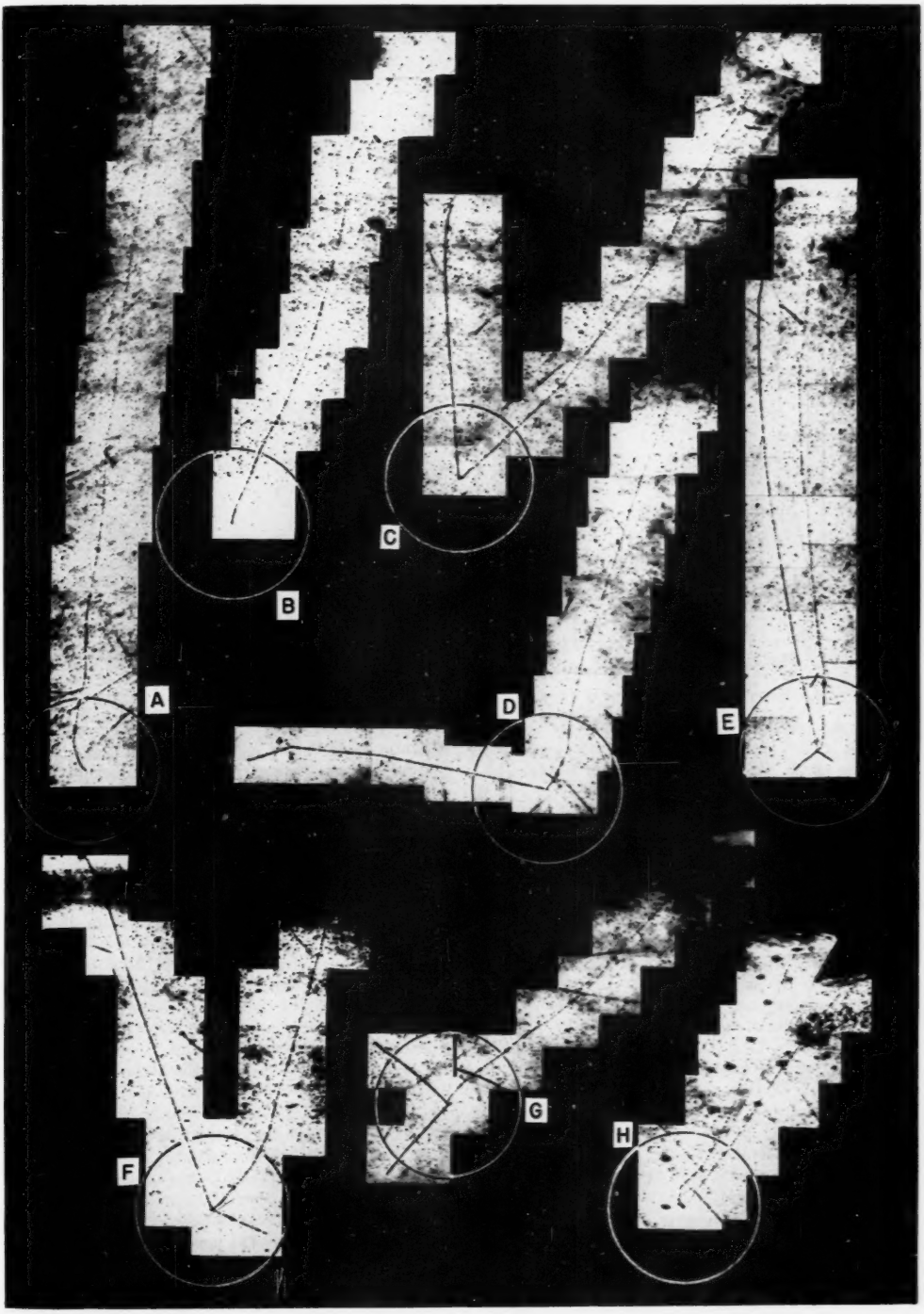
In making this study, those events were termed stars in which at least one track could be discerned leaving the terminus of the meson with a well-defined direction. A star was considered to have a recoil as one of its prongs when at least one of its branches was a "heavy" track less than five microns in length, with a definite direction. However, a poorly defined group of grains, showing no preferred direction, was not considered a prong, but was called a "club." A club may, in fact, be a nuclear recoil, but this interpretation is not certain. Mesons that ended within five microns of either emulsion surface were discarded in order not to include stars for which one or more prongs might be unobservable. Photomicrographs of typical meson events are shown in Fig. 2.

In this study 512 π^- mesons were observed to end in the emulsion. Their prong spectrum is shown in Fig. 3. The 137 mesons that appear in the first column did not initiate stars. Fifty-two of these formed clubs, as indicated by the blacked-in portion of this column. A count was not made of clubs associated with stars of one or more prongs. Shading in the other columns indicates stars that had a recoil prong as one of their branches. It was found that 26.8 ± 2 percent of the π^- mesons ending in the emulsion do not initiate observable stars. For all mesons in this study, the average number of prongs per meson is 1.7 ± 0.1.

Earlier prong spectra, taken with a different geometry, indicate that there is no statistically significant difference between Eastman NTB, Ilford C.2, and Ilford C.3 emulsions, and that there is also no significant difference between 50- and 100-micron emulsion thicknesses.

FIG. 2. Typical π^- meson events. Mesons enter at top of each photomicrograph. Each circle is 100 microns in diameter. (A) Meson ending with no event associated; (B) meson forming a "club"; (C) 1-prong star; (D) 1-prong star; (E) 3-prong star; (F) 2-prong star; (G) star with 3 prongs, one of which is a recoil prong; (H) star with 2 prongs, one of which is a recoil prong.

[1] We are indebted to Prof. Ernest O. Lawrence for his continued interest in this work. We also wish to thank Prof. R. L. Thornton and Drs. Eugene Gardner and C. M. G. Lattes for much helpful discussion. The authors are very grateful to F. M. Smith and D. J. O'Connell for microscopic work, to A. J. Oliver for the photomicrographs, and to James Vale and the cyclotron crew for making the bombardments. This work was done under the auspices of the Atomic Energy Commission.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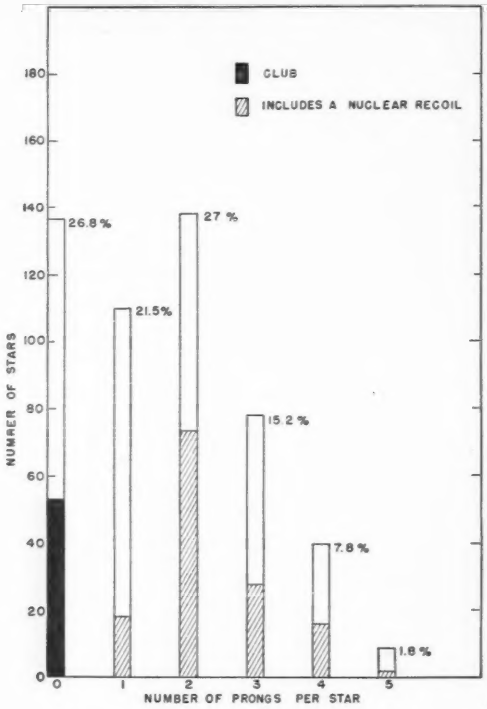

FIG. 3. Prong spectrum of 512 π^- mesons.

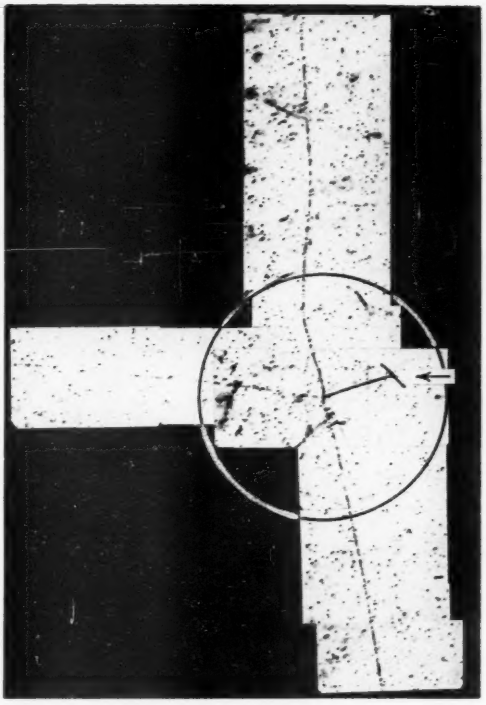

FIG. 4. "Hammer" track (A) from star initiated by π^- meson entering from the top.

The prong spectrum in an electron-sensitive emulsion (Eastman NTB3) was studied by Ellen Grunwald at the suggestion of W. H. Barkas. By grain counting, she found that in 64 π^- meson stars, no protons of energy greater than 40 Mev (or heavier particles of corresponding grain density) were emitted. Her prong spectrum was consistent with ours within the poor statistics. This agreement was to be expected, since no particles were found of energy greater than that registered by Ilford C.2 emulsions (protons of 50–60 Mev). A few slow electrons were observed, but no high energy electrons were seen. Since low energy electrons often could not be reliably assigned to an event because of heavy background of low energy electrons, their frequency of occurrence is not estimated here. This work is being continued by one of us (F. L. A.) using Ilford G.5 emulsion.

The decay of a π meson at rest into a μ meson has been observed in photographic emulsions (*11*). A significant question has been whether this process is restricted to positive mesons. In the more than 4000 π^- mesons that have been observed at this laboratory to end in the emulsion, only two events have been found (by W. H. Barkas and H. Bradner) which may be interpreted as negative π–μ decays. However, the possibility that these are stray π^+ mesons has not been ruled out; the probability is even larger that they are decays of π^- mesons which take place before the π^- meson comes to rest. It follows that, since the emulsion consists of discontinuous grains of AgBr and gelatin (*14*), π^- mesons seldom, if ever, undergo π–μ decay either in gelatin, which consists of light elements, or in AgBr. This substantiates the assumption of Lattes *et al.* (*12*) that the π–μ decays observed in cosmic ray photographic emulsions are decays of positive mesons.

Among 3000 meson events, eleven "hammer" tracks were found. A photomicrograph of such an event is shown in Fig. 4. These hammer tracks are attributed to the emission of a Li^8 nucleus as a star prong (*6*). The Li^8 decays into Be^8 with a half-life of 0.88 sec, and the Be^8 disintegrates into two alpha particles of equal energies with a half-life[2] of the order of 10^{-21} sec. The presence of these hammer tracks implies that the emission of fragments other than alpha particles and nucleons might occur with significant probability. This conclusion is not inconsistent with the observation of deuterons and tritons in high energy nuclear reactions induced by the Berkeley 184-inch cyclotron (*4*). Since it is extremely difficult to distinguish these heavier nuclei from alpha particles in the emulsions used, and since an unknown number of neutrons is emitted from each star, the detailed calculation of the energy given up to the star by the meson is impracticable in most cases.

[2] Calculated from energy spread given by T. W. Bonner *et al.* (*3*).

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Experimental Hypertension Produced by a Plastic Capsule Applied to the Kidney

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Various methods (*1*, *2*, *3*) have been described for producing experimental hypertension, including application of cellophane or silk around the kidney, and constriction of the renal artery. These methods, however, have limitations when applied to small mammals. The writer has found that an effective plastic capsule can be produced around the kidney by spraying that organ, when properly exposed, with a solution of plastic. This procedure is outlined briefly here.

Dogs were anesthetized with sodium pentobarbital 32.5 mg/kg intraperitoneally. Using the retroperitoneal approach, the kidney was exposed and gently lifted out of the body cavity. The fat and fascia surrounding the kidney were carefully removed, whereas the true renal capsule was removed in some cases and not in others. Aseptic technique was employed at all times and the operated animal was given regular doses of penicillin until the wound had completely healed. The operation was performed unilaterally. Two weeks later the procedure was repeated on the opposite side.

Plastic (butyl methacrylate polymer, procured from the E. I. du Pont Company) dissolved in acetone (200–250 ml per 100 g plastic) was sprayed over the kidney. This plastic solution hardens quickly into a tough capsule which is not disturbed by body fluids, body temperature, or movements of the animal.

A large atomizer was used to spray the plastic. The spraying apparatus must be washed with acetone to keep the openings patent.

Blood pressure in animals having plastic capsules applied to both kidneys by this method persisted at levels of 190 mm Hg and above. The rate of rise in blood pressure essentially followed that previously described by Page (*2*, *3*) when cellophane was used.

As control against the possible effects of the acetone in which the plastic was dissolved, tests were made using comparable quantities of acetone alone. No effects on the animals were noted.

The plastic may also be dissolved in toluene. The hardening process naturally takes a little longer, but the spraying apparatus is not as difficult to keep clean and patent.

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Absorption Effects in Volume Irradiation of Microorganisms

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Three types of experiments are generally performed to obtain the survival curves of organisms subjected to radiation: surface irradiation, volume irradiation, and volume irradiation with stirring. From the shapes of these curves it is possible to consider this process in terms of the target theory (*1*). It is difficult to attach physical significance to target area in the case of ultraviolet irradiation. Nevertheless, the relative target areas obtained from an action spectrum indicate the probability of an inactivation as a function of wavelength. In volume irradiations, unless the absorption of the medium and organisms are taken into consideration, erroneous values may be obtained for the relative target areas.

The standard single-hit target theory expression for the survival curve of irradiated microorganisms (*2*) is:

$$\frac{N}{N_0} = e^{-SB} \qquad (1)$$

where N_0 is the original number of organisms, N the number of survivors, S the target area, and B the dosage per unit area. This relation applies rigorously for the cases of surface irradiation and volume irradiations in which the total absorption of radiation by the suspending medium and suspended particles is negligible.

In general, actual irradiations are volume processes with finite absorption. We may distinguish two cases: case I, in which the organisms are fixed in the suspending media; and case II, in which the organisms are constantly stirred.

Case I. Monochromatic radiation is absorbed according to Beer's Law.

$$\frac{B}{B_0} = e^{-\alpha x} \qquad (2)$$

where B_0 is the incident intensity, B the intensity at any point x, and α is the absorption coefficient. Consider an

[1] The author wishes to express his thanks to Mr. Donald Fluke, who developed the point of view expressed in equations (11) and (12), and to Dr. Franklin Hutchinson for many valuable suggestions.

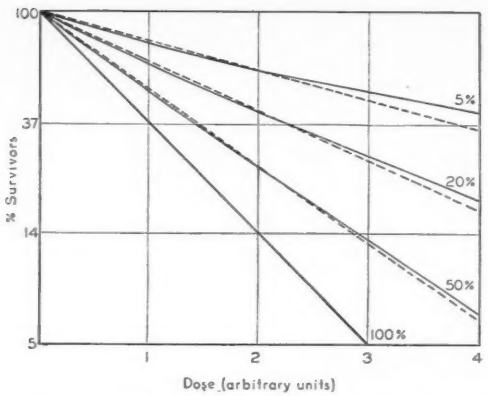

FIG. 1. Solid lines are predicted curves; broken lines are the straight line approximations. Labels on curves indicate percent transmission of irradiated sample.

absorption cell of area A and length L. This may be divided into a number of smaller volumes Adx. Equation (1) may be written for each of these small volume elements as follows:

$$\frac{\varrho(x)}{\varrho_0} = e^{-SB_0 e^{-\alpha x}} \tag{3}$$

where ϱ_0 is the original number of organisms per unit volume and $\varrho(x)$ the number of survivors per unit volume at x. This may be rewritten as follows:

$$\frac{LA\varrho(x)\,dx}{LA\varrho_0} = e^{-SB_0 e^{-\alpha x}}\,dx \tag{4}$$

and integrated as x goes from 0 to L

$$\int_0^L A\varrho(x)\,dx = N \text{ (total number of survivors)} \tag{5}$$

$$LA\varrho_0 = N_0 \text{ (original number of organisms)} \tag{6}$$

We then have

$$\frac{LN}{N_0} = \int_0^L e^{-SB_0 e^{-\alpha x}}\,dx \tag{7}$$

which may be put in terms of the standard integral[2]

$$\frac{N}{N_0} = \frac{1}{\alpha L}\int_{SB_0 e^{-\alpha L}}^{SB_0} \frac{e^{-u}}{u}\,du \tag{8}$$

Plotting log N against B_0 for various values of αL we get a family of very nearly straight lines until we get to very low survival as shown in Fig. 1. Provided we know αL, which can be obtained from the percent transmission, we may reduce all data to equivalent surface radiation by multiplying B_0 by an appropriate correction factor (see Table 1). This gives us the dose necessary to give the same inactivation if the organisms were spread on a surface. The correction factor depends on the percent of radiation transmitted through the actual irradiated sample, and is therefore a function of the wavelength, the material irradiated, and the depth of this material.

Case II. In a stirred suspension, equation (8) holds only for a very short time, dt, after which stirring gives a uniform distribution in which N_0' is now equal to N after time dt. If we suppose the total irradiation time is τ, the total surface dose is then given by $B_0'\tau$ where B_0' is the rate of irradiation. If dt is given by $\frac{\tau}{g}$ where g is a very large number, we get the following expression for the number of survivors

$$N = N_0\left[\frac{1}{\alpha L}\int_{\frac{SB_0'\tau}{g}e^{-\alpha L}}^{\frac{SB_0'\tau}{g}} \frac{e^{-u}}{u}\,du\right]^g \tag{9}$$

For complete stirring $dt \to 0$ or $g \to \infty$. Taking the limit of expression (9) as $g \to \infty$ we get

$$N = N_0 e^{-SB_0}\left(\frac{1-e^{-\alpha L}}{\alpha L}\right) \tag{10}$$

This time the plot of log N against B_0 gives a family of exact straight lines in which B_0 can be multiplied by a correction factor to give the equivalent surface dose. Equation (10) may be derived more simply by assuming that the organism is just as likely to be found in a given part of the cell as in any other part. The radiation it receives is thus the space average of the radiation throughout the cell.

$$B_{AVE} = \frac{1}{L}\int_0^L B_0 e^{-\alpha x}\,dx = B_0\left(\frac{1-e^{-\alpha L}}{\alpha L}\right) \tag{11}$$

$$\frac{N}{N_0} = e^{-SB_{AVE}} = e^{-SB_0}\left(\frac{1-e^{-\alpha L}}{\alpha L}\right) \tag{12}$$

Thus the results of both types of volume irradiation may be expressed as equivalent surface radiations when B_0 is corrected in the appropriate manner.

TABLE 1

Transmission	Correction factor (stirring)	Correction factor* (no stirring)	Maximum error due to straight line approximation for case I
%			%
100	1	1	0
90	.95	.94	5
80	.90	.89	5
70	.84	.83	5
60	.78	.77	5
50	.72	.71	5
40	.66	.65	5
30	.58	.56	10
20	.50	.46	10
10	.39	.35	10
5	.32	.27	15

* The correction factors for the nonstirring case are applicable, within the limits of error given, for survival from 100% to 37%. For lower survival rates (especially for high absorption) data must be compared directly to equation (8).

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Seasonal Variation in Sensitivity of *Rana pipiens* to Chorionic Gonadotropin

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Early in 1948, Wiltberger and Miller (*5*) and Robbins and Parker (*3*) independently announced the usefulness of the male *Rana pipiens* as a test animal for detection of chorionic gonadotropin of pregnancy. Both of these reports cited the use of small amounts of native urine. Subsequently Cutler (*2*), Brody (*1*), and others have reported the necessity of using concentrates prepared from large amounts of urine (20–50 ml) in order to obtain satisfactory results.

TABLE 1

SEASONAL VARIATION IN RESPONSE OF *Rana pipiens* TO CHORIONIC GONADOTROPIN

Season (1949)	No. of tests	Refractory animals	
		No.	%
Winter	866	2	0.2
Spring	1376	14	1.0
Summer	367	41	11.2
Fall	620	3	0.5

Our experience with the *Rana pipiens* test, which covers several thousand cases, correlates these discrepant recommendations. The animal is quite sensitive to chorionic gonadotropin during the fall, winter, and spring seasons. In this period it responds to between two and five international units of the hormone. The injection of a small amount of native urine is then sufficient, in most cases, to yield a positive response. In the summer, on the other hand, the animal is relatively insensitive, requiring from 10 to 25 units of gonadotropin for stimulation of spermatogenesis. Under these circumstances, the concentration method is essential to detect the chorionic gonadotropin, if present in low titer.

It is always necessary, and especially during the summer months, to test with known gonadotropin all animals which give a negative response in a test. For this purpose 1 ml of a concentrate prepared by the Scott (*4*) method from pooled pregnancy urine, containing about 50 I. U. of chorionic gonadotropin, is used in the summer season. At other times, 0.2 ml, diluted with saline to 1 ml, suffices. During the summer about 11% of the animals are refractory to gonadotropin, requiring retesting of the unknown specimen with another animal. In other seasons only about one animal in two hundred is refractory.

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Behavior of Hydrogen Ion in Exchange Reactions

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The anomalous behavior of hydrogen in ion-exchange reactions is well known. For example, when the adsorption energy of hydrogen ion is compared with other monovalent ions in clays and synthetic resins, it is found that hydrogen occupies a position which is quite abnormal in view of its charge and atomic weight. Moreover, no ion-exchange formulations are found to describe satisfactorily the reactions of hydrogen ion.

A possible explanation of these facts is that adsorbed hydrogen in the exchange materials is partly bound by covalent as well as electrovalent bonds. It is therefore of considerable interest to attempt the calculation of the surface dissociation constant for this ion.

Let us imagine an adsorbent in its hydrogen form treated with an electrolyte solution such as KCl. Then the ideas just stated lead to the following equation:

$$RH \longrightarrow R^- + H^+ \text{ (e.a.)}^1 \text{ (surface dissociation)}$$

and

$$k_{RH} = \frac{[R^-][H^+(\text{e.a.})]}{[RH]} \quad . \quad . \quad . \qquad (1)$$

The quantities in brackets may be expressed in terms of milliequivalents per unit volume of suspension.

Let us now consider the exchange between the K ions and electrovalently bound hydrogen ions. That is,

$$H^+(\text{e.a.}) + KCl \longrightarrow K^+(\text{ad}) + HCl$$

and applying the mass action law:

$$\frac{[HCl][K^+(\text{ad})]}{[H^+(\text{e.a.})][KCl]} = \text{constant} = C \quad . \quad . \qquad (2)$$

We assume here by analogy with weak acids and their salts that all potassium adsorbed is held electrovalently. Then from equation 2,

$$[H^+(\text{e.a.})] = \frac{1}{C} \cdot \frac{[K^+(\text{ad})]}{[KCl]} \cdot [HCl]$$

$$= \frac{X}{C}$$

where

$$X = \frac{[K^+(\text{ad})]}{[KCl]} \cdot [HCl]$$

The quantity X can be obtained experimentally. Substituting the value of H^+(e.a.) obtained from equation 2 in equation 1,

$$k_{RH} = \frac{\frac{X}{C}\left[\frac{X}{C} + K^+(\text{ad})\right]}{[T - K^+(\text{ad})] - \frac{X}{C}}$$

$$= \frac{X}{C} \cdot \frac{[X + C \cdot K^+(\text{ad})]}{[C(T - K^+(\text{ad}) - X]} \qquad (3)$$

[1] H^+(e.a.) refers to the amount of electrovalently adsorbed hydrogen in the adsorbent phase.

where T is the total exchange capacity of the adsorbent.

By experimentally determining X at any two values of $K^+(ad)$ it is possible to solve equation 3 for k_{RH} and C.

From the experimental data obtained with the synthetic resin "Ion X," k_{RH} and C have been calculated to be 0.82 and 11.0 respectively. It has been found that these values are unaffected by variations in the amount of potassium adsorbed or the amount of HCl in the system. It may be stated that k_{RH} has the dimensions of concentration and is therefore dependent on the mass of the sample used. The value 0.82 is on the basis of meq. per gr of adsorbent.

When the value calculated for C, 11.0, is taken as a measure of the adsorption energy of electrovalently bound hydrogen, the element assumes a position which is much more reasonable with respect to other monovalent ions. This will be clear from the corresponding constants for alkali metal ions. Adsorption energies of alkali metal ions and electrovalently bound hydrogen ions on the synthetic resin "Ion X" are set out below for comparison:

H	0.09
Li	0.55
Na	0.74
NH	0.95
K	1.00
Rb	1.22
Cs	1.70

Similar calculations for other exchange materials are being carried out and will be reported elsewhere.

Location of the Syncline in Island Arc Structure

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The tectogene hypothesis of syncline development is one of the more promising contributions to modern stratigraphy. By it we are given a cause, a mode of operation, a set of mechanical principles, a schedule of events, and a more-or-less predictable synclinal pattern. It is tacitly, if not expressly, assumed that the foredeep is the geosynclinal structure in which sediments are accumulated, to be lithified and preserved through future ages. This, however, raises certain difficulties.

In the first place, modern foredeeps include some which have been filled; some, partly filled; and some in which, apparently, little sedimentation has taken place. This third case includes those island arc troughs which now have ocean-water depths measurable in thousands or even tens of thousands of feet. These are true ocean deeps, and when sediments accumulate here to any great thickness, they will be of the deep-ocean type.

It would seem that some of the troughs of the past would have been slow in filling, also; and that, if we accept the identity of the foredeep with the geosynclinal structure, we should find, basal in at least a few such cases, true deep-sea deposits. No such find has ever been reported.

In the second place, the island arc pattern, as developed in modern seas, is sharply defined and distinctive. If the foredeep is also the geosynclinal structure, synclinal sediments of the Paleozoic, for instance, should exhibit such a pattern.

In the third place, sediments of many of the best-known synclines of the past were derived from land masses located seaward. If the foredeep is the geosynclinal structure, however, modern island arc arrangement practically excludes the possibility of a sourceland on the convex rim of the trough.

The island arc proper is the only significant source of sediments until a much later time in the history of the area. While the deep is still present, sediments can move in only two general directions: (1) from the islands, toward the concave rim of the trough, and hence toward (but not to) the main ocean basin; and (2) from the islands, toward the continent, and hence into the *becken* which lies inside the arc.

This *becken* (or *meeresbecken*), being located distinctly on the shelf, can receive only shallow-water sediments. Its pattern and size are only slightly affected by the pattern and size of the island arc. And much of its sedimentary load can be derived from seaward: first from the islands of the arc, and later from the upbuoyed tectogene.

It is not suggested that no true foredeep sediments will ever be found. It is suggested that the shallow *becken*, rather than the deep trough, may have been the geosynclines of the past.

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A Rapid Method for Obtaining Clear Soil Extracts

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Barium sulfate is widely used for flocculation of suspended matter in soil extracts, mainly because it eliminates most of the equipment necessary for filter paper procedure, it saves time, and because of its inert characteristics it does not interfere with the ionic concentration of the soil solution. The phenomenon is purely physical, in that the finely divided white powder carries a charge which causes the oppositely charged particles to flocculate and then settle to the bottom of the tube by the action of gravity.

This process, however, is not entirely complete, as a small amount of the powder remains suspended. For this reason alone, a simple and useful method to obtain clear soil extracts is rendered useless when testing for potassium, sulfates, calcium, and chlorides by the customary method, because of interference by the suspended barium sulfate powder. Thus, a quantitative analysis for these ions is out of the question.

Finely divided wood charcoal has been commonly used for clarification purposes, and was found to function in precisely the same manner when mixed with barium sulfate in the proportion of one part powdered charcoal (various brands gave equally good results) to four parts of barium sulfate. The charcoal apparently collected the small amount of remaining barium sulfate by adsorption and settled out in a dark layer at the top of the soil column, leaving a clear aqueous solution which could be readily drawn off with a pipette for analysis.

It was further found that the use of the popular sodium acetate-acetic acid extracting solution used in soil testing tended to enhance this settling process rather than hinder it, since clear extracts were obtained 5–10 min sooner than with distilled water alone.

A series of tests were carried out, using 0.1*N* acetic acid buffered to pH 5.5 with sodium acetate. No change was noticed in the ionic concentration of the extract when compared with an extract not using wood charcoal powder. Good results were obtained with various types of soils, including those with high clay content.

The general procedure for conducting a barium sulfate-charcoal extraction is relatively simple: One level teaspoon of soil is put in a 15 cm × 15 mm test tube; one-fourth teaspoon of a 4:1 barium sulfate-charcoal mixture is added, and then 10 ml of the extracting solution. The proportion of soil to water is now approximately 1:6. The test tube is stoppered and shaken for 1 min. The tube is then placed in an upright position for 20 min to clarify. With a pipette, as much of the clear extract is carefully drawn out as is needed to deliver aliquot portions to a series of tubes set up for various soil tests. The only alteration of the procedure is in the colorimetric determination of pH, wherein distilled water is substituted for the extracting solution.

Crystalline Dihydrostreptomycin Base

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Dihydrostreptomycin (*1*) has been crystallized both as the sulfate (*2*) and as the hydrochloride (*3*). We wish to report also the preparation of crystalline dihydrostreptomycin-free base. Crystals of the base were obtained by converting relatively pure dihydrostreptomycin sulfate essentially to the free base, either by titration of a water solution to about pH 12 with barium hydroxide or by passing an aqueous solution over a strongly basic ion-exchange resin in the hydroxyl cycle—for example, Amberlite IRA-400 (The Resinous Products Division of Rohm and Haas Company). The free base was then precipitated from the aqueous solution with a water-miscible solvent such as acetone. The precipitation was in the form of an oil which gradually crystallized as needlelike crystals with some tendency to cluster in rosettes.

The crystallinity of this material was established both by microscopic examination and by x-ray pattern study. The x-ray powder diffraction data (see Table 1) were obtained on a Norelco diffraction camera of 114-mm radius using nickel-filter copper-Kα radiation ($\lambda = 1.5347$ A).

TABLE 1

X-RAY DIFFRACTION DATA OF DIHYDROSTREPTOMYCIN BASE

"D" values*	Relative intensities
15.6	0.25
10.5	1.00
9.53	0.63
6.28	0.13
6.11	0.13
5.76	0.25
5.40	0.13
4.98	0.25
4.67	0.38
4.32	0.38
4.02	0.06
3.86	0.13
3.35	0.13
3.26	0.13

* Interplanar spacing.

The biological potency of crystalline dihydrostreptomycin base against *E. coli* by the Food and Drug Administration turbidimetric test was somewhat low, being only 922 μg/mg (average 6 days' test). This crystalline base has no mp up to 300° C; it charred at 240° C turning black up to 300° C; it was reconstituted as a 1% water solution, giving a pH of 12. *Analysis.* Calculated for $C_{21}H_{41}O_{12}N_7 \cdot H_2O$: C, 41.93; H, 7.20; N, 16.30. Found: C, 41.73; H, 7.32; N, 16.35; SO_4, 0; Cl, 0.

[1] The authors wish to thank Miss Margaret Carter and Mrs. Ruth St. John for their technical assistance.

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Comments and Communications

More on Zoological Nomenclature

My attention has been drawn to the communication from the Steering Committee of the Washington Discussion Group in your issue of December 2, which severely criticized the International Congress of Zoology for having introduced certain reforms into the International Code of Zoological Nomenclature at its meeting held in Paris in July 1948 and the International Commission on Zoological Nomenclature for having advised the congress so to do. This communication contains misstatements on questions of fact which, unless immediately corrected, might easily confuse the real issue involved and thus cause harm to the interests of international cooperation in the field of zoological nomenclature.

(1) The first criticism advanced by your correspondents is that the decisions taken by the congress were taken without preliminary consultation and without an opportunity's being given zoologists generally to express their views. This is not in accordance with the facts. Particular care was taken to gather together, by correspondence or otherwise, the views of zoologists before the opening of the congress. In the case of zoologists in the United States, the steps taken included the special visit paid by myself in the winter of 1947–48 (reported in your issue of December 12, 1947) during which, as secretary to the International Commission, I held a three-day conference at Princeton, New Jersey, with the American members of the commission, followed by a series of conferences, which lasted nearly a week, held at the Smithsonian Institution with your present correspondents, and other conferences with leading American zoologists at New York and Chicago and with paleontologists at Ottawa during the annual meeting of the Paleontological Society. These conferences were all concerned with the subjects to be considered at the Paris congress six months later. It was largely on the basis of these discussions that the agenda for the Paris session was drawn up. Throughout the Paris session, the commission met in public and its meetings were widely attended by zoologists from many countries. In every case affecting either the wording of the International Code or the composition, procedure and functions of the International Commission, the recommendations ultimately adopted by the commission were unanimous, as also was the subsequent endorsement of those recommendations, first by the Section on Nomenclature and second by the congress itself. Whenever there arose an issue on which there was any sign of disagreement, that issue was postponed for further consideration. To argue that the congress was misguided in adopting changes that were unanimously recommended to it by all the zoologists who took part in the work on nomenclature at the Paris congress is therefore tantamount to an uncompromising opposition to all reforms of every sort, even those against which not a single voice, even from among the representatives of what is now the Washington Discussion Group, was raised in opposition.

(2) Your correspondents seek to draw a distinction between the permanent members of the commission on the one hand and, on the other hand, the alternate members who, in accordance with established practice, were summoned to the membership of the commission for the duration of the congress in replacement of those of the permanent members of the commission who, by reason of the high cost of travel or prior engagements, were unable to take their places when the commission assembled in Paris. No such distinction is permissible, the congress having made formal provision, as far back as its Budapest meeting in 1927, for the appointment of alternate commissioners, with full voting rights, to replace commissioners who do not attend meetings of the congress. Equally inadmissible is the further contention advanced by your correspondents that matters of importance should be considered by the commission only by correspondence conducted between members of the commission during intercongress periods and that the proceedings of the commission at its actual meetings should be reduced to a formality. For that contention ignores the fact that the commission is a subordinate body of the congress, by which it was established, and that it would accordingly be totally wrong for the commission to adopt a procedure which would make it impossible for it to discharge the duties with which it is charged, when it met during sessions of the congress. Quite apart from this fundamental objection to the procedure advocated by your correspondents, it will be obvious to anyone possessing any experience in the conduct of discussions regarding intrinsically difficult questions that the chance of obtaining a solution by correspondence is extremely remote, the only satisfactory procedure for dealing with such problems being full and free discussion round the table.

(3) I note that at the conclusion of their communication your correspondents go so far as to suggest that in the future reforms of the code should be made not by the congress but by the permanent membership of the commission, acting necessarily by correspondence only, the function proposed to be reserved to the congress being the purely formal role of giving its approval automatically to whatever had been settled behind the scenes during intercongress periods. Few zoologists would regard this suggested procedure as an improvement on the democratic method of free discussion which has hitherto prevailed. Certainly no one can seriously suppose that the commission would lend itself to a revolt of this kind, against the body by which it was set up, still less that the congress would tolerate for an instant such usurpation of its rights.

(4) Zoologists will be glad to learn that, within the next few weeks, the minutes of the meetings of the commission in Paris will be published in the *Bulletin of Zoological Nomenclature*, in accordance with the di-

rections given by the congress. These minutes will contain a full and detailed record of the proceedings in Paris and particulars of every decision then taken on the recommendation of the commission. A study of this record will enable all who are interested in zoological nomenclature to judge the merits of the individual decisions then taken. It was the hope of the Paris congress that anyone who could advance serious grounds in favor of amending or expanding any of those decisions would submit a reasoned statement of his views to the commission, so that those views might be considered during the present intercongress period with a view to the formulation by the commission of suitable proposals for consideration by the Copenhagen congress in 1953. Prior to the Paris congress some of your present correspondents made a number of valuable suggestions which, on the recommendation of the commission, were approved by the congress and will accordingly find a place in the revised text of the code which, in accordance with the decision of the congress, will be promulgated at the earliest possible date. It is to be hoped that further valuable suggestions will in due course be made by the zoologists who now protest against the action of the congress in giving its unanimous approval to recommendations in the preparation of which they had themselves played a leading and important part.

FRANCIS HEMMING

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The concern of our sister nomenclature discussion group in Washington over the course of procedure at the 1948 meeting of the International Commission on Zoological Nomenclature in Paris—if the procedure was correctly understood and reported by them—should be shared by systematists generally. This question aside, however, their protest appears to disclose a regard for the past history of nomenclatural practice and procedure, and a concern lest these be changed that the Chicago group[1] does not share. It seems to us that nomenclatural discussion and publication since the turn of the century has to a considerable measure failed to attain the end of a stable nomenclature.

The fable of the Gordian Knot and of Alexander's solution for its unravelment is peculiarly appropriate to the one really basic issue, which is the desirability of conserving familiar names and avoiding confusing changes. Some systematists have sought to achieve stability by strict and unyielding application of the "law" of priority, regardless of its effects on widely used names. Others, finding vast areas of descriptive anatomy and zoological classification in need of critical work, wish to get forward with what nomenclature is necessary and are willing, in order to conserve such names, to resort to the sword of fiat decision, an instrument that the commission has the power to wield. We believe that Secretary Hemming is inclined to favor its use, and the Chicago group wholeheartedly concurs.

The whole business of elaborate argument over rules and "validity" of names has been a disgrace to zoology and has contributed more than any other single factor to the low repute of systematics among zoologists as a whole. Any system seeking continuity rather than change would have been infinitely preferable to the elaborate search for priority that was established as an international game by the early operations of the commission. No effort was made to establish a nomenclature of the familiar animal types once and for all; and the rules for fixing genotypes were made vague instead of clear, thus permitting wide latitude of opinion in their interpretation. So complicated were they that even Stejneger, a leading nomenclaturist and member of the commission, came up with a surprising number of wrong answers.

Thus the fear of our sister group that "the accomplishments of years in the field of zoological nomenclature will be jeopardized" seems, in the light of current arguments as to the generic name of the lion and of the common eel, to be somewhat disingenuous. Their further fear that "confidence in the International Commission may be lost" is unrealistic, for that confidence has long since been lost: witness the contempt in which the operations of name-changing are held by embryologists, anatomists, ecologists, and others who have a right to expect some measure of common sense from systematists. It will not do to maintain an attitude of aloof contempt for those "who don't know anything about nomenclature." It is admitted by the nomenclaturists themselves that nomenclature is merely a tool, a servant of biology, and not a dictatorial separate entity. Confidence in the commission can, in our opinion, be restored only by prompt and decisive action, and by a reorientation of outlook and procedure based on conservation and continuity, and not on change and confusion.

The fundamental requirement is a reformation in attitude. Strict following of the law of priority, regardless of the consequences, must be set aside as the guiding principle in nomenclatural procedure, both in the thinking of systematists and in the working of the commission. In its stead, there must be substituted a proper regard for the convenience of zoologists generally and a determination, by stabilizing names in current use, to avoid change and chaos. The method lies ready to hand in the device known as the Official List. Any name placed on this list *must not be subject to change for purely nomenclatural reasons.* The Official List has been little used in the past and needs to be greatly expanded. In order to accomplish the required expansion, machinery must be set up whereby, before placing names on the list, the commission can obtain, with reasonable rapidity, a genuinely representative sample of opinion from those workers most concerned and competent to

[1] An informal group associated with Chicago Natural History Museum constitutes a nomenclature discussion group in the Middle West. This statement is signed by W. J. Beecher, E. R. Blake, D. Dwight Davis, Henry S. Dybas, Robert H. Denison, Fritz Haas, Robert F. Inger, Heinz A. Lowenstam, Walter L. Necker, Everett C. Olson, Bryan Patterson, Clifford H. Pope, Austin L. Rand, Eugene S. Richardson, Jr., Colin C. Sanborn, Karl P. Schmidt, Charles H. Seevers, Rupert L. Wenzel, Loren P. Woods, Melvin A. Traylor, Jr., and Rainer Zangerl.

judge. We strongly believe that a wider participation in both the machinery and the selection of the commission by systematists is desirable. There are heartening indications in the recent work of the commission that a trend in this direction is setting in and it is earnestly to be hoped that it will continue.

As here envisaged, the expanding Official List would progressively become a sort of 20th Century *Systema Naturae*, a development that should be welcome to all those who regard nomenclature as a means to an end and not as an end in itself.

KARL P. SCHMIDT, for the
CHICAGO DISCUSSION GROUP

Chicago, Illinois

On the points set forth in paragraph 2 by the Nomenclature Discussion Group in Washington we should like to make the following remarks.

(1) The set of stated principles were by no means unknown to zoologists in general, since a great part of the work of the commission was directed toward an incorporation in the *Règles* of the earlier opinions. . . .

(2) Action was taken on individual amendments . . . in order to clarify the *Règles* in cases where the meaning of the present text is ambiguous.

(3) It was entrusted to "jurists" to draft the new formulation of the *Règles* in close cooperation with a special drafting committee consisting of representatives of the commission. The draft so produced is then to be distributed among all the commissioners and alternate commissioners for a three-month study. . . .

(4) The reorganization of the commission is intended to make it easier for interested—and even for opposing—groups of zoologists to be represented and to take an active part. . . .

At the Paris meeting we, the undersigned, represented the Scandinavian zoologists as regard nomenclatorial matters, and we are convinced that an overwhelming majority of these scientists will agree that, after forty years of attempting to follow the strict rules of priority, it has proved impossible thus to stabilize nomenclature except perhaps in a few groups such as birds. So a strong feeling has arisen that the commission should be allowed to work more promptly and on a broader basis. In Scandinavia, therefore, the results obtained at the Paris meeting have been fully accepted and warmly welcomed.

HENNING LEMCHE and RAGNAR SPÄRCK

Zoologisk Museum
Copenhagen, Denmark

The Steering Committee of the Washington Nomenclature Discussion Group in effect condemns the International Commission on Zoological Nomenclature, and especially its secretary, unheard. Both of the undersigned were present as alternates at the Paris meeting of the commission, and it may, therefore, be of some value to other zoologists if we give our views of what took place there. In the first place, we did not recommend to congress "the rewriting of the rules around a set of stated principles"; we took a great number of precise decisions as to the nature and form of amendments to be made to the rules, leaving only the exact wording to be decided upon later. The principles of the rules remained unaltered. We took "action on a large number of individual amendments" because, being in fact, not merely in name, the commission, it was our duty to do so, indeed, it was for the purpose of taking such action that the commission met; to adopt the course suggested by the Steering Committee would entail that the commission should vote twice on each proposal—once by post and once in session, a most curious and novel idea. We certainly did decide, and most properly in our view, that, having tied up all our decisions as carefully as we could, we would leave it to somebody more competent than ourselves, somebody trained to the task, in fact a jurist, to translate these decisions and amendments into formal language, subject to adequate safeguards; if the Steering Committee can suggest a better, quicker, and more practical way of dealing with this tedious and specialized task, we should be interested; on the other hand zoologists as a whole will, we feel sure, be grateful to have been relieved of a task which very few indeed would claim to be competent to discharge. We did, it is true, recommend congress completely to reorganize the commission, and for this long overdue decision we very gladly accept our share of responsibility. This action should surely be a matter for congratulation; it passes our understanding that it should be a cause of complaint.

The Steering Committee asserts as one of the "basic issues" of its plaint that all this action was taken without any preliminary announcement; it is true that in respect of some few matters no formal printed statement was published to the zoological world setting forth in detail precisely what the commission proposed to do at its Paris meeting. On the other hand, we believe that the members of the Steering Committee, through personal contact with the secretary of the commission the previous winter, were in fact very fully informed . . . and freely endorsed those plans. . . . The second "basic issue" we have already dealt with in the preceding paragraph. The third appears to us to consist of a complex of confusions. To say that there is "no provision" for the congress to review the work of the commission is nonsense; congress controls the commission. Zoologists present at the congress had on this occasion for the first time double opportunities to review the work of the commission; they could (and many did) attend its meetings, and they could have raised any issue they wished at the final plenary session. The one member of the Steering Committee who attended all these meetings contributed nothing by word or deed to any of them. The "regular commission" to which the Steering Committee so often refers is in a different position, for it is always, either by direct discussion when in session, or by post between sessions, able to review its own work to its heart's content, we presume. There are not two commissions, as the Steering Committee would lead us to believe, a competent "regular" one which is continually in existence, temporarily replaced at congresses by a specious substitute, but only one commission

throughout. The invidious innuendo contained in the committee's references to "regular" commissioners we most strongly resent. . . . We venture to doubt whether the opinions expressed in the Steering Committee's article are generally accepted even in the United States. They are certainly not shared by zoologists in Britain nor, so far as we can ascertain, in any other country.

EDWARD HINDLE[2] and N. D. RILEY[3]

London, England

Allow me, please, as a commissioner of the International Commission on Zoological Nomenclature, and as an Italian zoologist, to express my point of view.

My task is of course somewhat difficult, because of the fact that the N.D.G. has assumed the advantageous position of attacker; it makes some bold affirmations, without giving any proof of the truth of what it says, and it puts opponents in the position of being obliged, in answering, to prove what they say.

In the first place the N.D.G. in emphasizing that the International Commission, at the Paris Session, consisted of four regular members and eight or nine alternates, implies a doubt of the capacity of alternates to fulfill their task. The alternates were not appointed without previous consideration; the commissioners, if unable to take part in the congress, had been invited themselves to nominate their alternates, who, therefore, ought to be prepared for their task, not only because of their personal competence, but also by reason of instructions received from the commissioners.

The N.D.G. states that "the most momentous actions of a half-century in zoological nomenclature were taken at Paris." This implies, to persons not acquainted with the facts, that the International Rules of Zoological Nomenclature were, at least, basically changed. Nothing of the sort. Such change of the rules was out of the question. It is true, indeed, that the actual wording of the rules often permitted different interpretations and that many articles seemed partially to contradict others. These deficiencies in the rules are well known to every zoologist, and have been discussed many times, so that every zoologist might reasonably be expected to be acquainted with them. It is these deficiencies that the commission tried to eliminate at Paris, giving to the rules a clearer wording.

It is not true that actions were undertaken without notice to zoologists or to commissioners. Mr. Hemming, the secretary, had an extensive correspondence with the commissioners . . . many cases were actually resolved on the lines suggested by Dr. Blackwelder, the secretary of the N.D.G. . . .

The N.D.G. states, finally, that "if [such actions] are allowed to stand, international cooperation in nomenclature will be a farce, the confidence upon which support of the commission has been based will be lost." I find that it is rather a farce that a group of zoologists of a single country (not *all* the zoologists of that country, because there were many American zoologists, both in the Commision and in the Nomenclature Section, who fully agree with the action of the Commission) dares to speak in the name of "international cooperation" against the work of a commission where eight nations (U.S.A., the United Kingdom, Italy, Denmark, Netherlands, Belgium, Portugal, and Mexico) were represented, and which was supported by the approval of the Section on Nomenclature, where there were, in addition, Canadian, French, Swiss, and Egyptian members, and of the congress, where zoologists from all countries were present.

As far as the confidence of zoologists is concerned, I may say that, at least in my country, the commission was falling into the utmost discredit and zoologists were even coming to doubt the possibility of observing the International Rules, because of the well-known slowness with which the commission decided (or did not decide) cases submitted to it. The action of the commission in Paris has given Italian zoologists new confidence in the commission, as may be seen from two declarations, one made by the *Unione Zoologica Italiana* and the other by the *Societa dei Naturalisti di Napoli*, in which Italian zoologists express their wish that the revised rules may be published without delay; should the commission fail to do this, it would lose, I think, all its authority, at least among Italian zoologists.

LODOVICO DI CAPORIACCO[4]

Parma, Italy

As a commissioner present and voting in Paris, I wish to correct certain erroneous impressions given by the letter appearing in *Science*, December 2, 1949, from the Nomenclature Discussion Group, Washington, D. C.

The program presented by the secretary followed the lines endorsed at a meeting with the American Commissioners in 1947. Many details reflected the wishes of American zoologists as gathered by the secretary from correspondence and extended personal discussion not only with the Washington group, but at the Chicago meetings of the AAAS, at the Ottawa meeting of geologists in 1947, and with the staff of the American Museum of Natural History. Past congresses have taken actions importantly affecting nomenclature without such announcement.

The principles adopted were clear; if hastily worded at the moment, they would not have been clear. *Advisory* wording was left to jurists, actual and final wording to an editorial committee of the commission. . . .

The representation was widely international and included zoologists of eminence. There was prevalent a striking feeling of friendly international cooperation. Those present had the enthusiastic feeling that an important step forward had been made both in international good will and in the service of zoologists. Nothing was

[2] Scientific director of the Zoological Society of London, Fellow of the Royal Society, and chairman of the Zoological Section of the International Union of Biological Sciences.

[3] Keeper of the Department of Entomology, British Museum (Natural History), secretary of the Royal Entomological Society of London, of the Permanent Committee of Entomological Congress, and of the Entomological Section of the International Union of Biological Sciences.

[4] Member of the International Commission on Zoological Nomenclature, ordinary professor of zoology in the University of Parma.

reported to the congress that did not have the *unanimous* support of both the commission and the Section on Nomenclature.

Proof of the minutes of the session (approximately 650 printed pages), unavoidably delayed, is now in the hands of each commissioner, and after a short period allowed for approval will be at once published in the *Bulletin of Zoological Nomenclature*. The secretary suggested the Washington group await appearance of the minutes before they published anything. It is regrettable that they have not seen fit to do so.

J. CHESTER BRADLEY
New York State College of Agriculture at Cornell University, Ithaca, New York

A letter was also received from Pierre Bonnet, professor in the Zoology Laboratory, Faculty of Sciences, Toulouse, France, who expressed his astonishment at the views of the Washington Discussion Group and supported the decisions of the congress for reasons already stated in the other letters published here.

(*Continued from page 225.*)

2. *Clearance procedures should be confined to sensitive areas.* Attempts to extend security measures into nonsecret areas reflect the fear that dominates the attitude of large sections of the public and the press. The SCLP is convinced that such an extension of the security program would not benefit the national security and in fact would be harmful to the nation's best interests. In particular, it is hoped that no clearance will be required for nonsecret work under the proposed National Science Foundation.

3. *Scientists should take an active interest in the security program.* Much of the improvement in procedures evident since the war has presumably resulted from the strong recommendations made by scientists and others concerned. Individuals in universities and in industry can promote further improvements by familiarizing themselves with the clearance situation in their environment and by urging specific reforms.

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12. ——— *Bull. Atom. Sci.*, 1949, **5**, 299.
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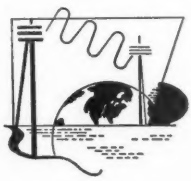

Book Reviews

Louis Pasteur: Free Lance of Science. René J. Dubos. Boston: Little, Brown, 1950. Pp. 418. $5.00.

Louis Pasteur: Free Lance of Science is no ordinary biography. It is science history and a story of the life of a man who left a deep imprint on scientific thinking and progress and an exposition of the philosophy of his time. The influence of Pasteur's heredity and his personality on his accomplishments is cleverly brought out. By drawing upon some intimate sketches of those people who influenced him most and by blending those sketches with Pasteur's contributions, Dr. Dubos has succeeded in producing a book that is at the same time both a fine addition to the history of science and a source of excellent entertainment.

Much of all the insight into Pasteur's life and times that the book affords is accomplished by sprinkling into the narrative carefully selected passages from his letters, speeches, and memoirs, and seasoning it well with pertinent quotations taken from utterances of his contemporaries and those of earlier scientists and philosophers.

Among these many quotations one may be mentioned that reveals the beauty of Pasteur's philosophy. "The Greeks have given us one of the most beautiful words of our language, the word 'enthusiasm'—a God within. The grandeur of the acts of men is measured by the inspiration from which they spring. Happy is he who bears a God within!"

Many of us who find burdens in and out of government predominately absorbing energies which would otherwise be employed in investigative pursuits must admire the man, Pasteur, who was also able to participate in the affairs of the community to the same degree and with the same driving spirit that characterized his behavior in the laboratory. "He managed to remain faithful to the laboratory while serving society" and was a "fervent scientist and an effective citizen," says the author. His profound sentimentality, the will to work, his urge to create and, finally, his indomitable will are all clearly portrayed to demonstrate the direction of his entire life and productivity.

Many of Pasteur's utterances find application today. We are quick to realize that times have not changed materially, except for the lapse in years. As an example, we find him quoted as follows: ". . . two contrary laws seem to be wrestling with each other nowadays: the one, a law of blood and of death, ever imagining new means of destruction and forcing nations to be constantly ready for the battlefield—the other, a law of peace, work and health, ever evolving new means for delivering man from the scourges which beset him."

In spite of his intense nationalism, Pasteur realized the international scope of science. He said, "Science knows no country because knowledge belongs to humanity, and is the torch that illuminates the world." Could he have been anticipating such possibilities as a National Science Foundation when he added, "Science is the highest personification of the nation because that nation will remain the first which carries the furthest the works of thought and intelligence"?

In speaking before the Academy of Sciences, in one of the many controversies he engaged in there, Pasteur revealed his penetrating awareness and analysis of the bureaucratic evils experienced in his day. What he said there serves to us a warning and incentive to fight such dangers abusive to our present system as we would fight the plague: "It is the subtle danger that arises from the assumption by an official body, however distinguished, of responsibilities beyond its real competence." This word of caution should be remembered by everyone upon whom responsibility rests in directing students or research, either by active administration or in the distribution of funds. How many young inspired students of science have become mere technicians through bad advice and unimaginative direction? How many scientific advances have been delayed through misdirection of funds that might have been available if those responsible for this distribution had had the vision and courage that might be reasonably expected of men in such positions?

Pasteur had a philosophy of research that many of us might imitate. "For him, experimental techniques and procedures of observation were never an end unto themselves, but only tools, to be used for the solution of a problem and to be abandoned as soon as more effective ones became available." He was not "satisfied with formulating the theoretical basis of the process . . . but took an active interest in designing industrial equipment. . . ." His well-known statement, "There are no such things as pure and applied science—there are only science, and the applications of science," is typical of his thinking. He also said, "It is not without utility to show the man of the world, and the practical man, at what cost the scientist conquers principles, even the simplest and the most modest in appearance."

Although the author does give emphasis to Pasteur's contributions to science and to mankind, he does not neglect to indicate that Pasteur had a personal side beset with many human frailties. Neither does he do an injustice to Pasteur's contemporaries by slighting either the importance of their contributions or the effect of the "laborious efforts of the 'unknown soldiers' of science" and philosophers who preceded him.

It is hard to realize that the infant biology only three quarters of a century ago was fraught with heated debates over the then highly controversial subjects of "spontaneous generation," the "germ theory" of disease, and the means by which practical immunization might be attained. Dr. Dubos has handled his subject matter skillfully. He has succeeded in convincing the reader of Pasteur's genius and the scientific productivity of his period, of the influences of his heritage on his

career, and the great influence of his work and philosophy on succeeding generations.

The book is to be highly recommended as being factually sound, intellectually stimulating, even exciting at times, and bright with human interest and personal anecdotes.

ROGER D. REID

Baltimore, Maryland

Einführung in die Zoologische Nomenklatur durch Erläuterung der Internationalen Regeln. Second edition. Rudolf Richter. Frankfurt, Germany: Verlag Waldemar Kramer, 1948. Pp. 252. 8.50 DM.

In this *Introduction to Zoological Nomenclature* Dr. Richter, for many years a member of the International Commission on Zoological Nomenclature and a distinguished paleontologist and nomenclaturist, has presented the most complete and best organized study of the subject that this reviewer has seen. The major portion of the work, after an introductory discussion (69 pp.) on general questions of nomenclature, is devoted to the articles of the *International Rules.* Each is discussed in turn, often in considerable detail and with numerous examples, in a plan reminiscent of, but more elaborate than, an early work by Stiles (1905, "The international code of zoological nomenclature as applied to medicine," *Hygienic Lab. Bull.* 24). In final sections, the author has listed the generic names that have been placed on the "Official List," genotype designations passed upon by the International Commission, and published works that have been suppressed for nomenclatorial purposes.

Although the discussions are of course not official interpretations, nomenclaturists will find much of interest in the author's views, especially on such subjects as family names, the Law of Priority, and homonymy.

CURTIS W. SABROSKY

U. S. Department of Agriculture

Scientific Book Register

External Morphology of the Primate Brain. Cornelius J. Connolly. Springfield, Ill.: Charles C. Thomas, 1950. 378 pp.; illustrated. $10.00.

Introduction to Theoretical Physics, 5 vols. Max Planck. Reissue; translated by Henry L. Brose. ***General Mechanics,*** Vol. 1, 272 pp. ***The Mechanics of Deformable Bodies,*** Vol. II, 234 pp. ***Theory of Electricity and Magnetism,*** Vol. III, 247 pp. ***Theory of Light,*** Vol. IV, 216 pp. ***Theory of Heat,*** Vol. V, 301 pp. New York: Macmillan, 1949. Illustrated. $3.00 a volume.

Medical Entomology. 2nd ed. Robert Matheson. Ithaca, N. Y.: Comstock Publ., 1950. 612 pp.; illustrated. $7.50.

Quantitative Ultramicroanalysis. Paul L. Kirk. New York: John Wiley; London: Chapman & Hall, 1950. 310 pp.; illustrated. $5.00.

Physiology of Heat Regulation and the Science of Clothing. L. H. Newburgh, Ed. Philadelphia-London: W. B. Saunders, 1949. 457 pp.; illustrated. $7.50.

Earth Waves. L. Don Leet. Cambridge, Mass.: Harvard Univ. Press; New York: John Wiley, 1950. 122 pp.; illustrated. $3.00.

The Sandhill Cranes. Lawrence H. Walkinshaw. Bloomfield Hills, Mich.: Cranbrook Institute of Science, 1949. 201 pp.; illustrated.

The Transuranium Elements: Research Papers. Glenn T. Seaborg, Joseph J. Katz, and Winston M. Manning. New York: McGraw-Hill, 1949. 1733 pp.; illustrated. 2 vols., $15.00 a set.

Physiology of the Eye: Optics. Vol. 1. Arthur Linksz. New York: Grune & Stratton, 1950. 334 pp., illustrated. $7.50.

Metabolism and Function: A Collection of Papers Dedicated to Otto Meyerhof on the Occasion of His 65th Birthday. D. Nachmansohn, Ed. New York: Elsevier Publ., 1950. 348 pp.; illustrated. $7.00.

Franciscan Awatovi: The Excavation and Conjectural Reconstruction of a 17th-Century Spanish Mission Establishment at a Hopi Indian Town in Northeastern Arizona. Ross Gordon Montgomery, Watson Smith, and John Otis Brew. Cambridge, Mass.: Peabody Museum, Harvard University, 1949. 361 pp.; illustrated. $5.85 paper; $8.35 cloth.

The Sea Shore. C. M. Yonge. London, S.W.1: Collins Publ., 1949. 311 pp., illustrated. 21 s. net.

Malariology: A Comprehensive Survey of All Aspects of This Group of Diseases from a Global Standpoint, 2 vols. Mark F. Boyd, Ed. Philadelphia: W. B. Saunders, 1949. 1643 pp.; illustrated. $35.00 a set.

Vitamin Methods, Vol 1. Paul György, Ed. New York: Academic Press, 1950. 571 pp.; illustrated. $10.00.

Terrestrial Radio Waves: Theory of Propagation. H. Bremmer. New York: Elsevier, 1949. 343 pp.; illustrated. $5.50.

Principles of a New Energy Mechanics. Jakob Mandelker. New York: Philosophical Library, 1949. 73 pp.; illustrated. $3.75.

Chemical Constitution and Biological Activity. W. A. Sexton. London, W.C.2, Engl.: E. & F. N. Spon, 1949. 412 pp.; illustrated. 55s. net.

Introductory Botany. Alexander Nelson. Waltham, Mass.: Chronica Botanica, 1949. 479 pp.; illustrated. $3.75.

Geology Applied to Selenology: The Sbrunken Moon, IV. J. E. Spurr. Winter Park, Fla.: J. E. Spurr, 1949. 207 pp.; illustrated.

Die Anaskosporogenen: Hefen, 2. H. A. Diddens and J. Lodder. Die Hefesammlung des "Centraal-Bureau voor Schimmelcultures": Beiträge zu einer Monographie der Hefearten. New York: Interscience; published in Amsterdam, 1942. 511 pp.; illustrated. $9.00.

NEWS and Notes

George B. Pegram, vice president of Columbia University, has been appointed scientific and educational consultant to the **Oak Ridge** Institute of Nuclear Studies. Dr. Pegram has been active in the atomic energy program since its inception, and has been on the institute's board of directors since it was organized in 1946.

The 1949–50 Jesup Lectures of the Department of Zoology, Columbia University, will be given during April by **S. E. Luria,** Indiana University bacteriologist. His subject will be "The Reproduction of Viruses."

Stanley W. Olson, who has been a member of the staff of the Mayo Foundation since 1946, and assistant director since 1947, was recently appointed dean of the Medical School of the University of Illinois. Dr. Olson will succeed **John B. Youmans,** who has accepted the position of dean of medicine at Vanderbilt University, Nashville, Tennessee. In addition, Dr. Olson will serve as medical director of the university's Research and Educational Hospitals.

Richard Foster Flint, professor of geology at Yale University, has been elected a Foreign Member by the Geological Society of London.

W. S. Tillett, professor of medicine, New York University College of Medicine, will give the sixth Harvey Lecture at the New York Academy of Medicine, March 16, on "Experimental and Clinical Studies on the Enzymatic Lysis of Fibrin and Inflammatory Exudates by Products of Hemolytic Streptococci."

A. Vincent Osmun retired February 1 as head of the department of botany at the University of Massachusetts. Dr. Osmun joined the staff in 1903 and became head of the department in 1914, succeeding the late George E. Stone. **Theodore T. Kozlowski** has been appointed acting head of the department.

A. E. Bennett, formerly professor of neuropsychiatry and head of the department at the University of Nebraska College of Medicine, has been appointed associate clinical professor of psychiatry at University of California School of Medicine.

Michael Cefola, formerly research associate with General Electric's Knolls Atomic Power Laboratory at Schenectady, has joined the faculty of Fordham University as assistant professor of analytic chemistry.

Ernest Sachs, of Yale University Medical School, will give the annual James Greenwood Lecture at the University of Texas Medical Branch, Galveston, on April 10, and at Baylor Medical College, Houston, April 11. His subject will be "The Evolution of Surgery of Brain Tumors."

Visitors to U. S.

V. Prelog, professor of chemistry at the Eidg. Technische Hochschule in Zurich, is presenting the Reilly lectures during February and March at the University of Notre Dame. His subjects are "Ionone Derivatives from Natural Sources," "New Developments of *Strychnos,* Yohimbé, and Cinchona Alkaloid Chemistry," and "Ring Formation and Ring Properties." Dr. Prelog will also lecture at other universities during his stay here.

Recent visitors at the National Bureau of Standards were **Nye E. deMers,** of the Alpha Institute Registered, Ltd., Montreal, Canada; **Kiyoshi Ishikawa,** electrical engineer with the Tokyo Shibaura Electric Company; and **S. S. Shrikhande,** assistant professor of mathematics, College of Science, Nagpur, India.

Grants and Awards

The **Anthony F. Lucas Petroleum Medal** for 1950 was awarded to William E. Wrather, director of the U. S. Geological Survey, and treasurer of the AAAS, at the annual banquet of the American Institute of Mining and Metallurgical Engineers in New York City, February 15. The medal is awarded in recognition of distinguished achievement in improving the technique and practice of finding and producing petroleum.

A **$12,000 scholarship fund** has been established at the University of Chicago Medical School in memory of the late Henry R. Boetteher, surgical specialist, who was a member of the first staff of Chicago's Englewood Hospital and a professor at Rush Medical College. Recipients of the first scholarships granted from this fund are Lawrence S. Sonkin, Robert S. Goldsmith, and Franklin D. Sidell.

A **Frederick Gardner Cottrell** grant of $3,500 has been awarded by the Research Corporation to Edward A. Fehnel, of Swarthmore College, for support of a research project on the preparation and properties of organic sulfur compounds.

Marshall R. Urist, of Los Angeles, received the **Kappa Delta award** at the annua dinner of the American Academy of Orthopaedic Surgeons, held in New York City on February 15. The award includes a prize of one thousand dollars and is to be given each year by the Kappa Delta sorority for outstanding research in orthopaedic surgery. Dr. Urist was honored for his research on the mechanism of endosteal bone formation.

Thomas M. Rector, vice president of General Foods Corporation, and chairman of the Panel on Food, Committee on Equipment and Materials, Research and Development Board, will receive the **Nicholas Appert Medal** for 1950. The Institute of Food Technologists will make the award at its Decennial Conference in Chicago, May 23.

Fellowships and Prizes

The **American Bureau for Medical Aid to China** is offering a number of fellowships for 1950–51 to Chinese citizens now in the U. S., who are holders of professional degrees and desire further training, or students in training for professional degrees. The fields covered are medicine, including the basic medical sciences, public health, nursing, dentistry, medical technology, nutrition, and hospital management. **Monthly** stipends and tuition fees are covered

by the grants. Applications must be in by *April 30*. Full information and application blanks may be obtained by writing to the organization at 1790 Broadway, New York City.

A fellowship for postdoctoral research in the natural sciences related to medicine and health has been established by Merck and Company, in recognition of the contributions to science of Selman A. Waksman, Rutgers University microbiologist. Citizens of the U. S. or Canada, holding the Ph.D. or equivalent degree, are eligible for the award of $3,000 for one calendar year, beginning July 1. Application forms may be obtained from the Executive Secretary of the Graduate Faculty, Rutgers University, New Brunswick, New Jersey, and must be returned by *March 15*.

The **Massachusetts Institute of Technology** is offering a number of fellowships in the field of electronics, sponsored jointly by a group of industrial organizations. Applicants for the graduate fellowship must fulfill requirements for admission to the graduate school, and a few advanced research fellowships will be awarded to candidates holding the Ph.D. degree or equivalent. Stipends for the graduate fellowships will vary from $1,200 to $1,800, and for the research fellowships from $2,400 to $3,600, depending upon qualifications. Applications should be made to the Director, Research Laboratory of Electronics, Massachusetts Institute of Technology, Cambridge, Massachusetts, at least four months before intended date of entrance.

The **Robert Roesler de Villiers Foundation** of New York City is offering a prize of $500 for the paper that makes the most significant contribution to knowledge of the nature, causes, origin, or treatment of leukemia. Papers to be considered for the award must have been published or accepted for publication before *October 20, 1951*. If the paper is considered to be of outstanding importance, the prize may be increased to a maximum of $1,000.

Colleges and Universities

Rutgers University's Serological Museum has added blood serum from sharks, skates, rays, and more than 30 other species to its present collection of serum from 60 species of marine life. Douglas G. Gemeroy, museum secretary and assistant professor of zoology, obtained these specimens during a recent expedition off the North Carolina coast, and further specimens are being collected by Harold Baier, instructor in zoology. Characteristics of the blood sera samples will be studied to determine the relationship of these fish and crustacea to other animals.

An institute of visual sciences, with the College of Optometry as nucleus, is being planned at **Pacific University**, Oregon. Courses in illumination and color, occupational vision, eye protection devices, optics, and biophysical research have been arranged for the 1950–51 academic year, in addition to the standard optometric courses. Plans are to extend the curriculum to cover all the physical, psychological, and physiological factors of light and sight. Services of the institute will be made available to education and industry.

Summer Programs

The Mt. Desert Island Biological Laboratory will offer a program in biological research this summer. In addition to general research two special programs will be offered—one in growth and tissue culture, the other in research training in renal and cardiovascular physiology. Applicants for the first course should write to Dr. Philip R. White, Institute for Cancer Research, Philadelphia 11, Pennsylvania. Applications for the second program should be made to J. Wendell Burger, Director, Trinity College, Hartford 6, Connecticut. Fellowship support is available from the Ulric Dahlgren Fund.

The Tissue Culture Association will sponsor a course of instruction in laboratory techniques required for tissue culture. This year, as in 1949, it will be under the direction of John H. Hanks, of the Harvard Medical School, and will be given in the laboratories of the Mary Imogene Bassett Hospital, Cooperstown, New York, from August 7 to August 31. Tuition is $50. The course is designed for postgraduates who plan to use cultured tissues in their research or teaching. Requests for application forms should be sent to Dr. Mary S. Parshley, College of Physicians and Surgeons, 630 West 168th Street, New York City, and should be returned to her by *April 15*. Successful candidates will be notified about May 1.

Meetings and Elections

The 16th annual meeting of the American Society of Photogrammetry was held in Washington, D. C. January 11–13. Delegations represented agencies in government mapping, private mapping organizations, manufacturers of instruments and equipment incidental to photogrammetry, educators, and interested persons in associated fields. Many members from foreign countries, including a number of students training in this country, attended the meeting.

A tour was made of the U. S. Navy Hydrographic Office and the National Bureau of Standards where methods of mapping production were observed, and studies and tests of photogrammetric equipment, optics, and physics were demonstrated.

The opening paper, "Mapping for Defense," was delivered by Herbert Milwit, of the Department of Defense. He stressed the importance of photogrammetry, recognized by the Department of Defense, and gave an outline on the current position of the department on matters pertaining to the science.

Robert N. Colwell, assistant professor of forestry at the University of California, talked on the use of aerial photographs in forest recreation, to develop much-needed trails and locate suitable camping sites. Managers of fish and game needing periodic inventories of resources have recently used aerial photos for census taking. Population counts have been successfully made from aerial photographs of waterfowl, deer, and even salmon.

Amrom Katz, chief of the Photo Physics Branch at Wright-Patterson Field, Dayton, Ohio, gave a paper on "Contributions to the Theory and Mechanics of Photo Interpretation from Vertical and Oblique Photographs," listing lines of attack which the Air Force is pursuing in

an attempt to solve the dual problem of collecting more information on and from the negative and print. He explained an extremely simple photo-interpreter's slide rule which can be used for image speed and cycling time calculations in aerial photography, and described a graphical computer for determining coverage and scale at any point in an oblique photograph.

Leslie E. Howlett, of the Canadian National Research Council, Ottawa, reported on the "Assessment and Calibration of Lenses for Air Photography" and stressed two matters of photogrammetric optics which are of great concern to photogrammetrists—lens-film resolving power and the calibration of the aerial camera.

A new approach to flight planning was discussed by Morris M. Thompson, of the U. S. Geological Survey, and a procedure for aerial photographic flights was developed from basic mathematical theory. The design includes allowances for the probable effect of relief, tilt, crab, and inaccurate flying. A major departure from previous practice is a new method in which the spacing is expressed as base/height ratio (B/H) for spacing along flight lines, and width/height (W/H) for spacing between flight lines.

C. A. VanDusen, of the U. S. Navy Bureau of Aeronautics, gave a short history of naval aerial photography in Alaska and the Arctic, and recent naval aerial surveys in that region.

Of particular interest was the article on "Photogrammetry in Astronomy," delivered by Father Heyden, director of the Georgetown University Observatory. He reviewed the development of astronomic photography from its beginning in 1850.

Electronics has its place in pursuing accurate measurement in photogrammetry. "Shoran for the Photogrammetrist," as described by Archer M. Wilson of the Engineer Research and Development Laboratories, Fort Belvoir, Virginia, showed recent developments in this field.

A new approach to vision testing for operators of photogrammetric equipment was presented by Max H. Salzman, of the Navy Hydrographic Office. His deductions are based on the concept that visual capabilities of individuals do not remain static. Periodic vision testing can bring with it more accurate and precise photogrammetric work.

The photogrammetrist has a tremendous stake in world food production in these days of global relationships. Charles E. Kellogg, of the Agricultural Research Center, Beltsville, Maryland, stated that a full realization of soils' potentialities depends upon full use of skills in the natural and social sciences, and in engineering. He explained how aerial photographs, and maps made from them are essential for economical and effective work in planning soil use programs and suggested that the society prepare standards for alternative methods to meet the problems in various countries.

The present and future possibilities for use of high altitude photography when applied to mapping and reconnaissance were discussed by Lewis A. Dickerson of the Corps of Engineers.

H. T. U. Smith, of the University of Kansas, illustrated his remarks on geology with a series of slides showing how prehistoric sand dunes could be detected with relative ease in aerial photographs.

J. V. Sharp, of the Bausch and Lomb Optical Company, listed some of the basic factors affecting the performance of optical instruments in photogrammetric equipment. These factors fall into three general groups and are concerned with map production, map accuracy, and map reproducibility. A lively panel discussion followed.

Several display panels illustrating maps and equipment produced or manufactured by leaders in the photogrammetric field were shown. Of particular interest was a huge 240-inch folded focal-length camera being made for the Air Forces by Boston University. It is to be used for high altitude photography.

Eastman Kodak showed a 16-mm sound film "Photography in Industry" and had samples of the new autopositive film that needs no negative to reproduce an image. Reed Research had a display of its new Focalmatic reflecting projector which automatically focuses the image as it reduces and enlarges. Fairchild Aerial Surveys Inc., and Aero Service Corporation had samples of airborne magnetometer maps, for use in oil and mineral exploration. Magnetometer surveys chart electrical impulses obtained from a gyroscopically operated "bird" suspended from an airplane. This company also had a sample of a regional relief model of the U. S. reproduced in color on plastic vinylite. Of additional interest to members was the showing of a two-projector instrument named "Twinplex" which was displayed by the U. S. Geological Survey. It is still in development and is not in established production, but has an adaptation for certain tasks. Two of its main features are the increased accuracy of measurement in the model and the increased size of the model which reduces costs of ground control surveys.

ROSCO J. FRENCH
U. S. Coast and Geodetic Survey

The Fluid Dynamics Division of the American Physical Society elected the following officers on February 4: chairman, J. G. Kirkwood; vice chairman, A. Kantrowitz; and secretary-treasurer, J. H. McMillen. The next meeting of the division will be held in Urbana, Illinois, May 12–13.

A series of lectures on problems of the Arctic will be held at Johns Hopkins University, under the sponsorship of the Arctic Institute of North America and the Johns Hopkins Isaiah Bowman School of Geography on the following dates: March 2—"Antiquity of Man and the Arctic," *George F. Carter*, professor of geography and director, Isaiah Bowman School of Geography; March 16—"Mineral Problems in the Alaskan Arctic," *John Reed*, staff geologist, Island and Territory Possessions, U. S. Geological Survey; March 30—"Glaciology, Glaciers, and the Arctic," *W. O. Field*, in charge exploration and field research, American Geographical Society; April 13—"Engineering Problems of the Arctic," *Capt. P. W. Roberts*, USN, Bureau of Yards and Docks, Washington, D. C.; April 20—"Problems in Government Adminis-

tration for the Welfare of Alaska's Native Peoples," *Margaret Lantis*, Grant Study, Harvard University; April 27—"Problems in Archeology of the Arctic," *Henry B. Collins*, senior ethnographer, Smithsonian Institution; May 11—"Ground Frost Phenomena in the Arctic," *A. L. Washburn*, executive director, Arctic Institute of North America, Montreal; and May 25—"The Arctic as a Strategic Scientific Area," *M. C. Shelesnyak*, director, Baltimore-Washington office of the Arctic Institute of North America.

The opening lecture of the seminar was given by Neal A. Weber, professor of zoology, Swarthmore College, February 16, on "Insects of the Arctic."

Lecture hours are 2: 00–4: 00 p.m., in The Rogers House, at the university.

The Division of Petroleum Chemistry and the Division of Physical and Inorganic Chemistry are planning to hold a joint symposium on the **Use of Isotopes in Petroleum Chemistry** at the American Chemical Society meeting in September. In order to allow time for preprinting, authors should submit two copies of papers and two copies of a hundred-word abstract by *May 15* to one of the symposium cochairmen, G. A. Mills (Petroleum Chemistry), Houdry Process Corporation, P. O. Box 427, Marcus Hook, Pennsylvania, or John Turkevich (Physical and Inorganic Chemistry), Chemistry Department, Princeton University, Princeton, New Jersey.

A two-day **symposium on chemical-biological correlation** will be held at the National Research Council in Washington, D. C. May 26–27. Papers scheduled for the first session are: "The Toxicity and Parasiticidal Action of Aromatic Arsenicals in Relation to their Structure," *Harry Eagle*, National Institutes of Health, and *G. O. Doak*, University of North Carolina; "Structure-Activity Relationships in Plant Growth Regulators," *A. Goeffrey Norman* and *Robert L. Weintraub*, Chemical Corps; and "Review of the Structural Requirements for Sympathomimetic Drug Action," *Alonzo M. Lands*, Sterling-Winthrop Research Institute.

The second session, explaining the work of the NRC Chemical-Biological Coordination Center, will include talks on the chemical and biological codes developed by the center for its punched-card files, the center's screening services, and discussions on methods of handling typical questions which come to the center. *David F. Marsh*, University of West Virginia, and *George Anderson*, American Cyanamid Company, will compare the relationship between structure and activity of antihistaminic and antithyroid compounds, respectively, as indicated by the center's punched card system, with conclusions derived from their previous studies.

A panel discussion on antimalarials is scheduled for the evening session of the first day of the meeting. *Robert C. Elderfield*, Columbia University, will be the moderator.

The first session of the second day will hear papers on: "The Relation of Structures of Diphenyl Derivatives to their Fungicidal Action," *James G. Horsfall*, Connecticut Agricultural Experiment Station; "Structural Changes in Plant Insecticides as Related to Insect Toxicity," *H. L. Haller*, U. S. Department of Agriculture; and "Effect of Drugs on Physiologically Active Thiol Systems," *Maxwell Schubert*, New York University College of Medicine.

At the last session *Harris L. Friedman*, Lakeside Laboratories, will present a paper on "The Influence of Isosteric Replacements on Biological Activity." A combined panel discussion on antimetabolites, carcinogenesis, and cancer chemotherapy will follow; *M. J. Shear*, National Institutes of Health, will be the moderator.

Names of the members of both the panel on antimalarials and the combined panel on antimetabolites and cancer will be announced later. It is expected that a symposium volume will be prepared and published after the meeting. Those interested in attending should register as soon as possible with the Symposium Committee, Chemical-Biological Coordination Center, National Research Council, 2101 Constitution Ave., N.W., Washington 25, D. C.

The May meeting of the New York Section of the International Association for Dental Research will be held on the evening of May 4 at the Guggenheim Dental Clinic, 422 East 72nd Street, New York City. Members and guests who wish to present papers at this meeting should communicate with the secretary, and abstracts should be in his hands by *April 20*. Abstracts of papers appearing on the program will be published in the *Journal of Dental Research*.

Deaths

William Crocker, the organizer of Boyce Thompson Institute for Plant Research, Inc., and managing director from 1921 until his retirement, September 1949, died February 11, in Athens, Ohio, at the age of 74. Dr. Crocker completed and published in 1948 *The Growth of Plants*, a summary volume of much of the work accomplished under his direction at the institute.

Herbert Dodge Pease, 79, died February 14 in Springfield, Massachusetts. Dr. Pease had assisted in founding the bacteriological laboratory of the Philadelphia Board of Health, and that of the Sheffield Scientific School at Yale. He organized the hygiene and antitoxin laboratories of the New York State Department of Health and directed both laboratories for ten years. He was head of the Pease Laboratories of New York City.

Frank Ellsworth Dodge, 83, chemical engineer known for his work on the recovery of coal by-products, died February 17 at Rockville Center, Long Island, New York. When he retired in 1946 he was with the Midwest Radiant Corporation as chemical engineer.

Karl Guthe Jansky, radio research engineer, died February 15, at 44. Mr. Jansky, who had been with the Bell Telephone Laboratories since 1928, discovered radio waves of extraterrestrial origin in 1933, and worked on radio direction finders during World War II.

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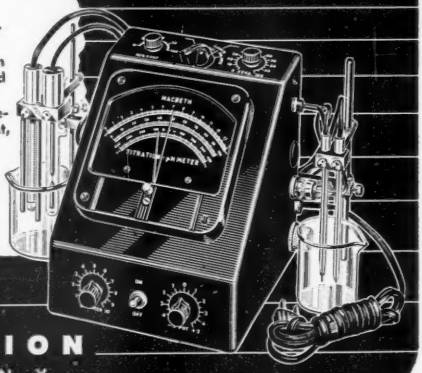

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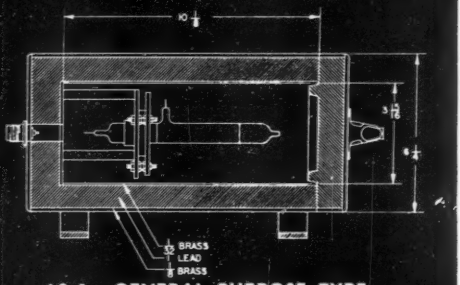

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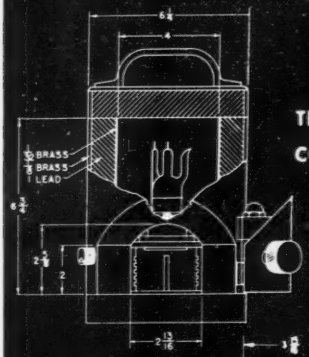

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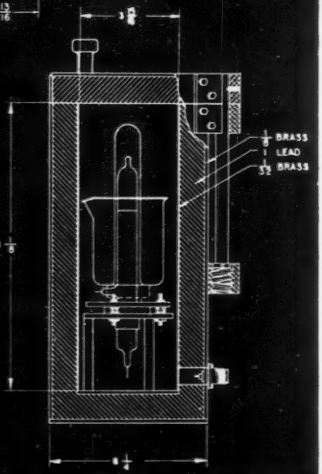

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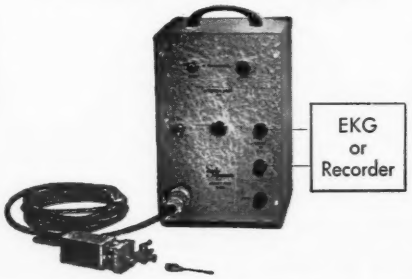

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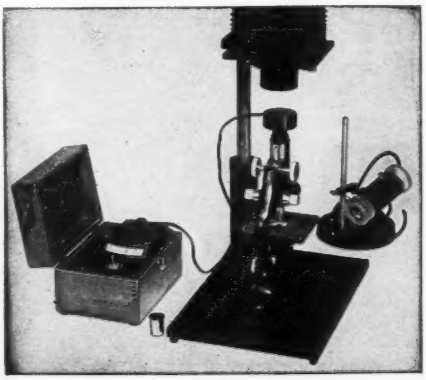

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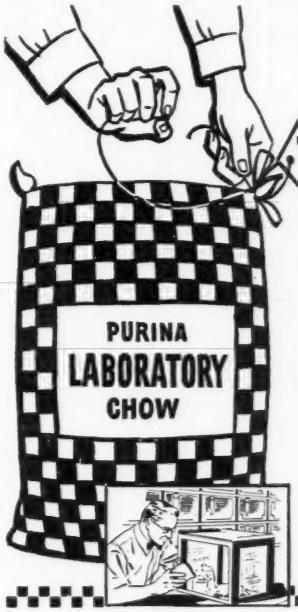

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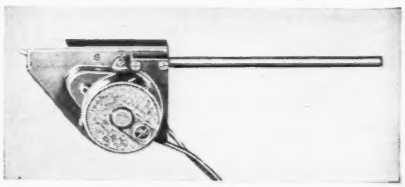

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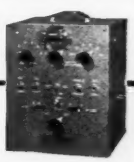

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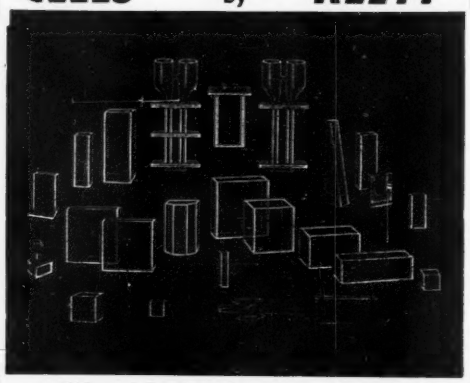

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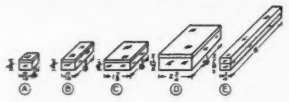

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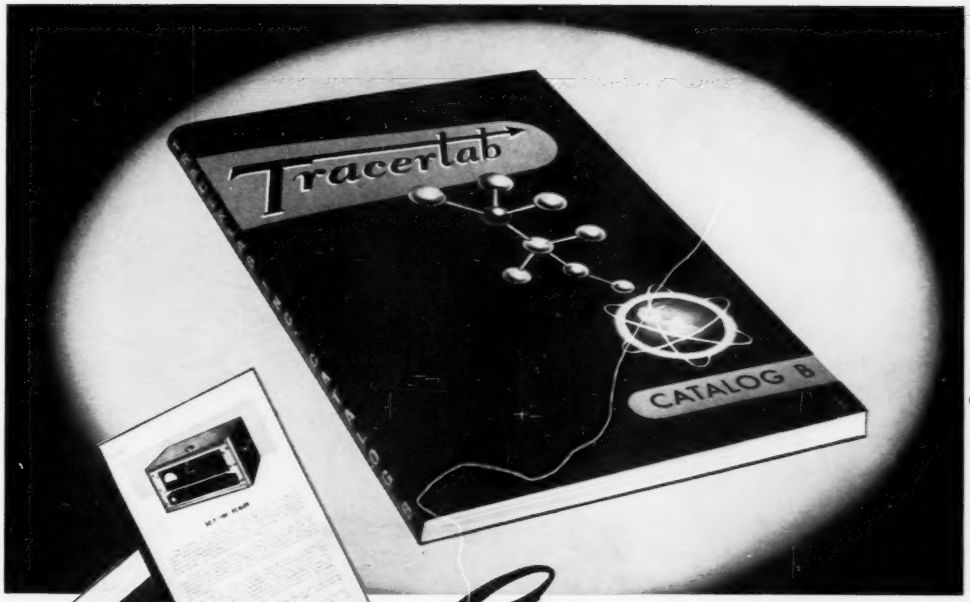

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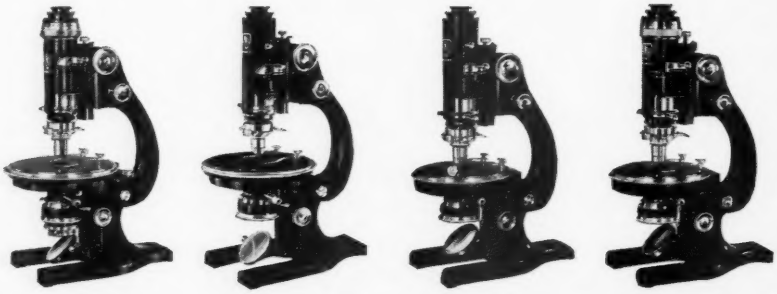

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